UNIVERSITY OF MARYLAND AT
COLLEGE PARK LIBRARIES

Digitized by the Internet Archive
in 2022 with funding from
Kahle/Austin Foundation

https://archive.org/details/makersofmodernec0001unse

THE MAKERS OF MODERN ECONOMICS

VOLUME I

ARNOLD HEERTJE

The Makers of Modern Economics

The Makers of Modern Economics

Vol. I

Edited by

Arnold Heertje

Mck
HB
76
.M33
1993b
Vol. 1

New York · London · Toronto · Sydney · Tokyo · Singapore

First published 1993 by
Harvester Wheatsheaf
Campus 400, Maylands Avenue
Hemel Hempstead
Hertfordshire, HP2 7EZ
A division of
Simon & Schuster International Group

© 1993 De Echte Kern bv

All rights reserved. No part of this publication may be reproduced, stored in a retrieval system, or transmitted, in any form, or by any means, electronic, mechanical, photocopying, recording or otherwise, without prior permission, in writing, from the publisher.

Typeset in 10/12 pt Times
by PPS Ltd, 174 London Road, Amesbury, Wilts.

Printed and bound in Great Britain by
T.J. Press (Padstow) Ltd

British Library Cataloguing in Publication Data

A catalogue record for this book is available from
the British Library

ISBN 0-7450-1513-1 (pbk)

---

1 2 3 4 5 97 96 95 94 93

Contents

List of contributors vii
Preface ix

1 **Partha Dasgupta**
Population, resources, knowledge and destitution: the making of an economist 1

2 **Takashi Negishi**
General equilibrium theory and the history of economics 47

3 **Ariel Rubinstein**
A subjective perspective on the interpretation of economic theory 67

4 **Roberto Scazzieri**
Actions, processes and economic theory 84

5 **Reinhard Selten**
In search of a better understanding of economic behaviour 115

6 **Joseph E. Stiglitz**
Reflections on economics and on being and becoming an economist 140

Index 185

Contributors

Partha Dasgupta is Professor of Economics at the University of Cambridge, UK.

Arnold Heertje is Professor of Economics at the University of Amsterdam, The Netherlands.

Takashi Negishi is Professor of Economics at the University of Osaka, Japan.

Ariel Rubinstein is Professor of Economics at Tel Aviv University, Israel.

Roberto Scazzieri is Professor of Economics at the University of Bologna, Italy.

Reinhard Selten is Professor of Economic Theory at the University of Bonn, Germany.

Joseph E. Stiglitz is Professor of Economics at Stanford University, USA.

Preface

In this book the active pioneers of modern economics describe their development as both scholars and teachers. They discuss those great researchers of the previous generation who helped to shape their careers as economists and all the contributors have provided their own views on the direction that economic theory will take in the future. The book is, therefore, a useful guide for the next generation of economists who, while standing on the shoulders of the present-day makers of modern economics, will find new theorems and new ways of understanding the economic landscape.

I have asked all the contributors to describe their own research and thinking and not to hesitate through fear of appearing a little less modest than one might expect. They have responded to my request in an open and frank manner. The advantage of this for us all is a clear picture of the significance of their research from their own point of view. This may represent the starting-point for others who may be at a point in their careers where a certain stimulus is needed.

I am grateful to all the makers of modern economics for co-operating in such a friendly way.

Arnold Heertje
June 1992

1

Partha Dasgupta

Population, resources, knowledge and destitution: the making of an economist

1. Entrance into economics

When I began graduate work in economics at Cambridge in 1966, growth and capital theories were showing signs of fatigue, but to the economic theorist they were intellectually still the most vital of subjects. Anything else, and you would be working on something less urgent, certainly something less meaty. If you wanted to make your mark as a theorist, this was the pair of subjects to work on. Or anyhow, that is how things appeared to the Cambridge graduate student and, judging by their publications, to those at MIT, Yale, Stanford and Berkeley as well.

Not that it was self-evident I should work in these loftiest of fields. For me to have thought it was would have required a degree of self-confidence I did not possess. I had the previous year's summer completed six uneasy years, during which I had not grown much. I had learnt techniques in both physics and mathematics (my undergraduate subjects at Delhi and Cambridge, respectively), but I had become bored with the seemingly endless preparation demanded of one for research in theoretical particle physics (which is what I was specializing in at Cambridge). So far as I could see I still had not reached the frontiers of the subject, and the last course I had taken, on scattering matrices, was a litany of computations with little in the way of coherence. There seemed to be no physics in it. At home my father had engaged in interminable conversations on economics with his colleagues and students. Observing them had instilled in me a desire for creative interaction with others. In those days physicists and mathematicians kept their counsel and listened. As an undergraduate at Cambridge I had thus spent little time with either physicists or mathematicians. With but two exceptions my closest acquaintances were in economics and philosophy. My friends among economists and philosophers, in contrast to the mathematicians, were never at a loss for words, and I began to understand something from them of the motivation underlying each discipline.

That was all I got from them though. Cambridge economics was charged with a desire to understand the dynamics of capitalism through growth and capital theory. Cambridge philosophy was involved in language games. I comprehended neither, so neither rubbed off on me.

On completing the final part of the Mathematics Tripos in 1965 I had little idea of what to do, other than spend some more years studying. The thought that I should do graduate work in economics came from two of my friends, Amit Bhaduri and Francis Cripps. What struck me about them was their ability to discuss economics problems effortlessly, which they did for hours on end. They were involved with their subject in a way I had never been with any subject. I envied them that. Of course, I realized they were gifted minds, but I thought their involvement reflected something inherent in economics, or anyhow in the style of doing research in economics.

Cambridge economics in those days was cloaked in Victorian morality. Your work would be taken seriously only if it had been prompted by an urge to do good to the world. You had to be serious in your social commitments. The thought that research in economics could be pleasurable, and that it could be the sole reason for your doing it would have filled Cambridge economists with horror.[1] If one desired intellectual enjoyment one should do mathematics; economics was only for the morally upright, I was told repeatedly by Joan Robinson, among others. This view seemed to me to reflect rather poorly on the subject, and I could not see why my teachers held it. In any case, my reading of the history of science was that moral motivation had little connection with research accomplishment. All sorts of hedonistic characters had made terrific discoveries. Goodness, as Mae West remarked in a different context, had nothing to do with it.

Among the first articles I read in economics were Robert Solow's 1956 paper on economic growth, and Edmund Phelps's 1962 paper on the golden rule. They were fun articles and they were intellectually serious. Moreover, even though unschooled in economics, I could sense their motivation. I could see that economics was going to prove enjoyable. But entry into it had nothing to do with my wanting to help the poor, or reduce unemployment, or understand capitalism. Although a Bengali, I had not read any Marx. Worse, I was not even a Marxist!

2. Preparation

Graduate students in Cambridge are required to have an official supervisor. This assignment is reached through informal matchmaking, and is then ratified by the Faculty Board. My options in the autumn of 1966 were James Mirrlees and Joan Robinson, as they were the two Faculty members to have expressed an interest in having me as their research student. Mirrlees had supervised my reading during the previous year while I prepared for undergraduate examinations in economics, but tutorials are not the occasion for a teacher to exhibit his creative talents. Mirrlees's formidable intellectual powers were yet to be on public display, and he was only a Lecturer in the Faculty of Economics. So I had no idea that he was a creative force. Robinson in contrast was world-famous; she had also belatedly just assumed her Chair (the one I hold now), and was a friend of my father. Nevertheless, Mirrlees seemed right to me, and for two reasons. First, although we were temperamentally far apart (he was spartan and morally upright), I *liked* Mirrlees. I was also grateful to him, for he had shown a personal interest in my moving to economics, and had arranged the necessary financial aid.

The second reason was less lofty. In the spring of that year I had met my future wife and had proposed to her on our first date. Even though she had not given a firm 'yes', I was not about to allow refusal to enter the agenda. She was an undergraduate at the London School of Economics (LSE) and was due to graduate in two years' time. If we were to marry I would have to get a job. (The thought that she might support me did not occur to either of us.) As I had no degree in economics, I was unemployable until I obtained a PhD, and I had a little under two years in which to get it. Mirrlees had come to economics via mathematics. So he could be expected to know the intellectual pitfalls facing someone like me.

There was an auxiliary reason tied to this. Mirrlees was an expert on the theory of optimal savings. About the one thing I had mastered during the previous year was optimal savings in an economy with one commodity. I reckoned that if I was to complete my thesis in less than two years it had to be on the theory of optimum economic development in a one-commodity world. With Mirrlees to guide me I thought it would be plain sailing. But it was not.

The problem was that when I thought about it I could not see what was left to be done on the one-commodity model. It seemed to me Tjalling Koopmans, Mirrlees and Carl Christian von Weizsäcker had between them dug out all the gold. I was wrong in this, of course: there were a number of excellent 'second best' questions to be asked within it. But I did not realize it at the time; my grasp of economics was too inexpert. Nor was Mirrlees about to suggest a problem to me, at least not in a language I understood. We met every week during that autumn term, and he talked, or rather he reflected verbally, in what appeared to me was an oblique language. It is possible he was suggesting problems to me. If he was, I failed to catch them.

At the beginning of 1967 Mirrlees informed me he was going on leave to help Pakistan for the coming three months, but that I was not to worry, because one of the world's outstanding economic theorists, Hirofumi Uzawa of Chicago, was to be a visitor at Cambridge and would be my supervisor during the coming academic term.

This was terrific news. Uzawa had by then published half a dozen articles on some of the hardest problems in economic theory (in one paper he had even worked out optimal economic development in a world with *two* commodities!), and he was reputed to be almost as good as Robert Solow with graduate students. So I was both excited and apprehensive when I went to meet him in his office. I had overlooked to make an appointment, and simply knocked on his door.

My first impression of Uzawa has remained with me even though we meet relatively often these days. He had what one can only call *presence*. His face reflected a languor akin to philosophic calm, and a restlessness which comes with problems remaining unsolved. But we did not get a chance to talk. After I had introduced myself and we had finished bowing to each other, the telephone rang. And it rang every time he put down the receiver. Uzawa's English, although clear, was slow and halting. I had also the impression that he in any case deliberated before saying anything. So each conversation was long. This went on for nearly two hours, at the end of which he suggested we go out to lunch. I thought that now at last I was going to be given a meaty problem in optimum savings. Alas, it was not to be.

By the time we had reached the restaurant we had discovered that we both opposed US involvement in Vietnam. Uzawa was very concerned about it. So was I. There was much about the war we wanted to discuss, so we talked about it over lunch. And we talked about it each time I went to meet him over the next eight weeks, which was invariably over lunch he bought me. All this was most agreeable, but it had got me no nearer a thesis topic. On what was to be my last but one visit to him at the end of that winter term I apologized for not having shown him any work (I had not written anything, I had nothing *to* write) but reminded him that as he was my supervisor he was expected to write a report on my progress to the Faculty administration, and so would he kindly do that before he left on his travels. To this request Uzawa consented, but expressed surprise. He told me he had had no idea he was my supervisor. (Either the Faculty administration had overlooked to inform him or, more likely, he had simply not taken note of it.) I realized then that he was under the impression I had visited him regularly only because we liked each other and had similar views on political matters. He had offered me friendship and I had taken him for a research supervisor. I felt a bit cheap.

Meanwhile, time was running out.

3. Optimal development with non-transferable capital

Round about that time Amit Bhaduri lent me a preprint of an article he had received from Sukhamoy Chakravarty on optimal development in a two-sector model with non-shiftable capital. He felt that as Chakravarty was his brother-in-law he ought to read it, but the mathematics in it was beyond him, and since I seemed to know all the mathematics any self-respecting economist would wish to know, perhaps I would explain the article to him.

I found the paper incomprehensible. It attacked a problem in the calculus of variations, but I had seen nothing like it before. Chakravarty was using techniques invented by someone named Valentine, which were unknown to me. Worse, I could not fathom Chakravarty's conclusions (which were correct, as I later realized). So I wrote up the problem in my own language and showed it to Bhaduri before making an attempt to solve it. The model was simple. It assumed that the economy was closed and had two sectors (producing consumption and investment goods, respectively). Once installed in a sector capital was not transferable to the other sector. Capital–output ratios in the two sectors were fixed. Labour was abundant and free, and the idea was to find the optimum pattern of investment over time when the planning criterion is to maximize the undiscounted sum of the flow of instantaneous utility from consumption over an infinite future.

Bhaduri assured me that the descriptive model in the exercise was of impeccable pedigree. I learned that the statistician and planner, Prasanta Mahalanobis, had imported it from the former Soviet Union for use as a basis for India's Second Five-year Plan. Bhaduri thought Chakravarty was the first to have tried optimizing within the model. As I did not follow Chakravarty's argument, I was confident he had got it wrong, but I had no idea how to solve the problem even after having reformulated it. Capital being non-shiftable meant use could not be made of the classical variational calculus, which was all the variational calculus I then knew.

There was no one in the Economics Faculty who knew modern variational methods (Mirrlees was away) and so I went to my old teachers in applied mathematics for help. They had not seen a problem like that either. As a last resort I went to Uzawa (who was back at Cambridge for a few days) and wrote the problem on the blackboard. He stared at it for a few minutes and told me to read someone named Pontryagin, who had invented variational methods for solving problems like the one I was studying. Unfortunately, before he could get up and solve the problem for me, his telephone rang.

It took me a month to get a grip on Pontryagin's techniques (his co-authored book, translated into English in 1962, was hard going), and then I started working on my problem. Meanwhile, it had got round in the Economics Faculty that I was optimizing on Mahalanobis's model. This cut some ice because the model was an Indo-Soviet invention. David Champernowne asked me if it was true I was doing a Ramsey on Mahalanobis. I told him it was, and asked him if he had an intuition on how the answer would go. To which he replied the whole matter was rather obvious. He said the thing to do was to pretend capital was transferable and work out the optimal development programme (for this problem one could use classical variational methods). Call this the 'unconstrained Ramsey path', he said. Should the ratio of the two types of capital stock inherited by the economy with non-transferable capital be off this path (which would almost surely be the case), the optimal policy would be to specialize all investment in the sector with the relatively little capital stock until the unconstrained Ramsey path were reached. From that point the optimal thing to do would be to follow the unconstrained Ramsey path.

Once Champernowne told me this it seemed intuitively obvious, and I felt hugely deflated. It now became clear to me that this was the result Chakravarty had established; he had got it right after all. But all was not lost. Chakravarty had assumed the instantaneous utility function to be isoelastic. It occurred to me concavity was all we were entitled to assume (in addition to the function being bounded above, which the formulation required), and so it was worth generalizing the result. Admittedly, it was not much of a thing to do, but it gave me *something* to do.

This was just as well, because I soon began to doubt Champernowne's intuition on the matter. This was because I could not reproduce Chakravarty's argument that along the unconstrained Ramsey path capital is never transferred across sectors. He had not drawn a phase diagram of the unconstrained Ramsey path on the space of the two capital stocks, and this had thrown me. When I reformulated the problem with my newly learnt tool-kit I made the unremarkable discovery that on this phase space the unconstrained Ramsey path had to be along a north-easterly direction if Chakravarty's solution was to be correct. It was now a simple matter to find the necessary and sufficient condition on the utility function for which this was so.[2] Were the utility function to violate the condition over an interval, the unconstrained Ramsey path would be 'backward bending', and would therefore be infeasible for the model with non-transferable capital. This meant that the optimal trajectory in the economy with non-transferable capital could involve repeated switches between specialization (in the consumption sector as it turned out) and expansions of both sectors. To calculate exactly when the economy should jump off the unconstrained Ramsey path and specialize required a delicate set of considerations, and I found that there were non-pathological cases where the economy might wish to display this switching behaviour indefinitely.

This began to look like a publishable paper to me (Dasgupta, 1969a). It was quite different from anything I had seen in the literature. Mirrlees, who had returned from helping Pakistan some time earlier, and had a chance to see my notes, said it was not unsatisfactory work. Coming from him, this meant I should receive a medal. Clearly I was in business, and so I began working on a richer model I had devised, in which labour was an explicit factor of production. (The point was to show that Mao's walking-on-two-legs technological policy could be optimal under certain circumstances; Dasgupta, 1969b.) Alas, my *joie de vivre* lasted only a short while, because from the mail Mirrlees soon began receiving it seemed everyone had been working on my problem. Unknown to us Michael Bruno, Leif Johansson and Harl Ryder had written papers on what was essentially the same model, and I learnt from Tony Atkinson (who had just returned from a year at the Massachusetts Institute of Technology, MIT) that Sanjit Bose, a student of Solow's, had completed a thesis on it. Worse, I learnt from him and Geoffrey Heal (who had returned from a year's stay at Berkeley) that to the graduate economics student in the USA using Pontryagin's method was like solving a quadratic equation. I was told that in some departments it was a prerequisite for graduate micro-economics.

Admittedly, Mirrlees assured me that none of the articles he had come across had my result in it (that an optimal trajectory can have blocked intervals), but the world felt crowded none the less. I also felt quite lonely, and began to wish I were somewhere else, such as Yale or MIT or Stanford, where techniques would be readily available, and where there would be graduate students to chat with on theoretical economics. As it happens, I did not know then that Kenneth Arrow and Mordecai Kurz were independently arriving at my result, but within the confines of a one-commodity world with irreversible investment. Had I known this, it would have been still more depressing. Even so, it had become clear to me that I had to find something quite different on which to work.

4. The Ramsey–Sidgwick–Meade problem

This something came in a letter from James Meade, in response to some notes I had prepared on Koopmans's and Peter Diamond's reformulation of Ramsey's criterion for intergenerational justice. Meade wrote (as always, in longhand) that while he had leant much from my notes (which I did not believe even then), it worried him that so much attention was being lavished on the problem of intergenerational savings, even though the most pressing problem facing the world was population. He wrote that the two questions were related: how much a country should save depended on how many people there were going to be, and how many people there should be depended on how much the country saved. He said that before the war he had regarded average utility per person to be the right criterion by which to judge policy, but that since the war he had had a change of mind; he now thought total utility of all who ever are to live was the morally right criterion. He also said he could not help stressing this was a most urgent matter, and that he would be very pleased indeed if someone were to solve the twin problems of optimum population and savings.

Needless to say, I had never thought about any of this. I had not even read Henry Sidgwick's *Methods of Ethics*, where the Classical Utilitarian criterion for population is laid out explicitly. But upon reading Meade's letter I knew I was going to work on it and solve it satisfactorily. I was temperamentally suited to it; it was my sort of stuff. Moreover, there was no evidence that either Koopmans or Solow had this problem on his research agenda. This meant no one was working on it at Yale or MIT.[3]

I decided to work on a one-commodity world in which aggregate savings are directly and fully controllable by the government (as in Ramsey's formulation), and where population is directly controllable subject to the limitation that its percentage rate of growth lies within a range (viz. between the net fecundity rate and the mortality rate). As for the optimality criterion, I followed the post-war Meade and considered the sum of the (possibly discounted) flow of total utility over an infinite horizon.

To me the first interesting result to emerge from my calculations was that the formulation demanded greater ethical underpinning than Ramsey's did: the origin of the utility function mattered. Economists were used to allowing the instantaneous utility function to be unique up to a positive linear transformation. Here, it could only be unique up to a proportional transformation, a stronger version of cardinality than usual. This was new to me. Unfortunately, it was as far as I got.

The prevailing standard for this sort of investigation in those days was that you had to prove an existence theorem (that an optimum programme over the infinite horizon exists), to be followed by a theorem which characterizes the optimal trajectory. If you were working with a model with only one or two commodities, the characterization theorem had to be sharp and complete. You could not simply say 'Here are the possible phases' or 'The optimal trajectory converges to this steady state'; you had to piece the phases together, as I had done in my earlier paper. Even after a number of attempts I was unable to obtain a full characterization of the optimum path for my new problem. I had the three phases: keep population growth at the minimum, keep it at the maximum, and keep it in between (the interior solution), but I could not piece them together. I had a horrible feeling that the solution may involve blocked intervals again. That would be most tiresome; I did not want to make the same point over and over again. (As it turns out, blocked intervals do not occur in that model, but I did not know it then.)

I struggled with the problem for some time, but then had to call a halt to it. I wanted to compete for a prize given each year to a doctoral candidate in the Economics Faculty. I decided to submit my completed paper on the Mahalanobis model together with my Maoist extensions. The latter had yet to be completed, and so it needed work.

By the time I could return to my population problem it was the beginning of 1968. I had two papers completed and needed a third for my PhD. My fiancée and I had decided to get married at the end of June. We then planned to visit my parents in India over the summer. I wanted therefore to have my oral defence of my thesis over and done with before getting married. This meant my dissertation had to be submitted by the middle of April at the latest, and the administration had to be informed now that I intended to do so. I struggled some more with my formulation before going to Mirrlees. This time he realized I really had given it all I had got. He suggested that for the purposes of simplification even population should be thought of as being directly and instantaneously controllable by the government. This meant that the two bounds on the rate of growth of population could be removed. When I reminded him that the world was not like that he told me I had missed the point. He said if I followed his advice there would be a clean, first-best problem for me: the point would be to treat population like a consumption good. He told me of the sorts of technical difficulties I was likely to face once the problem had been simplified his way, but added that he was confident it could all be done by spring.

It was at that meeting I realized how lucky I had been to have Mirrlees as my teacher. He had never given me a problem, which is what I had assumed a supervisor was supposed to do. On the other hand, he had been there to keep me from taking wrong analytical turns, which now appeared to me to be the greater gift. But it also meant he had been a few steps ahead of me at every stage. This was unnerving. I began to have ominous thoughts that he knew all the theorems I was ever going to prove before I had proved them.

It took me a month to complete the paper (Dasgupta, 1969c). By the time I was done with it I had developed an intuitive grasp of the area so complete that it chilled me. I found myself sketching (correctly) the solutions to extensions of my models which colleagues suggested to me even before they had completed their suggestions. In the central case (constant-returns-to-scale production in labour and capital) I showed that a (jointly) optimal population and savings programme exists if and only if there is positive utility discounting and the consumption level at which utility is zero (the *utility* subsistence rate) is not too low. In the way Mirrlees had suggested the problem be posed the economy's capital stock defined its initial condition (population was instantaneously controllable). I proved that in the central case the optimal policy (when it exists) is a steady state from the word go (this has been reproved by Barro and Becker, 1989, with a more general optimality criterion). I also showed that Phelps's golden rule of savings (all wages are consumed and all profits are saved) is *never* optimal. Were an economy to follow the rule which is prescribed for optimizing the population size (today this is called the Meade rule), utility would be zero at the golden rule consumption level, and this can always be improved upon by a change of policy. I showed that when there is a fixed factor in production (e.g. land), Classical Utilitarianism can imply a very large population size enjoying a consumption level only a tiny bit in excess of the utility subsistence rate, and that you could vary the parameters of the problem to make the optimal consumption rate come as close to the utility subsistence rate as you like. (Parfit, 1982, 1984, later christened this the 'repugnant conclusion'.)

For the first time in many years I felt free and happy. It was not only that I was soon to marry the girl I loved, liked and admired; I also experienced the pleasure of a genuinely creative exercise. Moreover, I had won for myself the Stevenson Prize of the University for an original essay in economics, and I had recently attained temporary financial security by my election to a Research Fellowship at Trinity Hall, a college which was to provide me with just the right kind of intimacy and support. It seemed to me there could not be too many people so lucky as I. Mirrlees told me my population paper was satisfactory. Coming from him this meant I deserved a Chair or something. I felt I knew more about the ethics of population and savings than anyone in the whole world, and if Meade thought it was the most pressing problem in the world, who was I to argue? Admittedly, there were many in the Economics

Faculty who thought it was only a bit of mathematics I had engaged in (they thought it meaningless to extend welfare economic theory to population), while some felt it was a mere extension of Ramsey, but I was not about to allow that to mar my spring. Most important, my father wrote saying he thought it was a good paper, and I trusted his judgement over anyone else's. He said it was nonsense to think welfare analysis should not be applied to population problems (Wicksell, Cannan, Dalton and Robbins had written extensively on the welfare economics of population). He also wrote to say that if serious economists had stopped writing on it some 40 years back it was only because they had run out of ideas on the subject; it signified nothing else.

5. PhD

My thesis examiners were Champernowne (internal) and Hahn (external). When submitting the dissertation I made the request that it be examined by the end of June, when I was due to be married. This gave the examiners little time over what is in any case a busy period involving undergraduate examinations. My examiners were at liberty to say no. In a gesture I subsequently discovered was characteristic, Hahn called me to say that he was my external examiner (officially he should not have communicated with me) and that he would see to it I was examined in time. In the event, my oral examination was held the day before my fiancée and I were married.

Frank Hahn in those days was an even more terrifying figure than he is now. Today his brilliance is tempered by a benevolent tolerance for the billions less gifted than he, but it was not then. He lived an academic life and had a command over the technical details of economics in a way which, as far as I can tell, was not surpassed by any economists of his generation. He was, and is, a complete economic theorist.

On entering Champernowne's office for my orals I found Hahn in a relaxed mood. This could only have meant he had found errors in my thesis, so I knew there was trouble. He said he did not think the thesis was bad, that he was recommending it for the degree, but the proof of a central lemma in Chapter 2 (the Mahalanobis model) was all wrong. 'However', he told me with a reassuring smile, 'the lemma is correct; I discovered a brilliant proof of it while having my bath yesterday and have written it down for you.' He then went to the blackboard and pointed to the step where I had gone wrong in my proof, and he then sketched his own proof.

I understood none of it. My mind was wrestling with something far more important: how on earth was I to type out the correct proof, remove the incorrect proof from the bound copies, insert the new pages and have the examiners' final approval, all before the following morning? On coming out of Champernowne's office I realized there was nothing to be done that day. There were a number of chores unconnected with my thesis which needed doing. I decided to investigate the 'Hahn problem' early the following morning.

I awoke at dawn and went through my own proof of the lemma, which I had been proud of: it was short and simple. There seemed to be no flaw in it. I studied it some more. Still no flaw. So then I turned to Hahn's proof, which was several pages long. As he had skipped steps along the way for me to fill in, I found it difficult to understand. It was only after some while it dawned on me that it was he who had got it wrong. There was a point in my proof where after a few steps of having established $x \geqslant y$, I had written $-x \leqslant -y$. In his rush to read my thesis before my wedding date Hahn had thought this was a wrong move!

I went immediately to see Hahn at his home. He appeared at the door in his dressing gown and with a morning pipe in his mouth. 'Dear boy', he said, 'you are over-excited and have come to the wrong place. Shouldn't you be getting married to that delectable English rose? She is far too good for you. As for me, I am preparing myself for the champagne reception to follow this afternoon at the Meade residence.' I ignored this and said he was wrong in thinking my proof was wrong. To which he replied, 'Partha my son, you must learn that Uncle is never wrong.' I did not wish to dispute this; I was going to be late for my wedding. And so, handing my own notes on the matter I rushed back to my rooms, where my best man was waiting for me. He reminded me that it was his job to take me to my wedding on time, that my fiancée would admonish him for being late. In the event I was late for my wedding. He was not admonished by my wife (she liked him too much to be angry with him), and she was understanding with me. But I have not been able to live it down. She still teases me about it.

That afternoon the first thing Hahn said on arriving at the wedding reception was that I was right after all. 'It only goes to show you learnt something when you took my general equilibrium course', he said.

Right or wrong, it did not matter: I had in effect got my PhD with an hour to spare.

6. Evaluation of public investment

The theory of optimum saving was regarded by most economists in England as esoteric stuff. Many thought it debased the intentions of the subject's founding fathers. Some even feared it signalled the end of political economy (I am using the term in the sense the British use it) and saw in it the beginnings of a decline of economics into pure mathematics. I heard a number of my teachers say as much openly. Admittedly, general equilibrium theory had preceded it, and had looked even more forbidding. But it had rapidly found a niche in the pages of *Econometrica*. Since most students never opened that journal, it had posed little threat. The theory of optimum saving (and growth theory more generally) was a different matter though. It was relatively simple to acquire the necessary techniques, and there had been a rush to the field among the young. Appropriately enough, much of it had been appearing in the *Review of Economic Studies*, which is a journal *of* the young.

Hidden behind its abstract mathematical form, however, the theory nurtured one deadly serious implication for applied economics: it provided the intellectual basis for the evaluation of public policies with long-run consequences. Originally, it had looked as though the theory's scope was limited to identifying optimal macro-economic policies. By the second half of the 1960s it was becoming clear that the theory not only provided the foundations for evaluating micro-economic decisions (such as public investments), it was also invaluable for the development of concrete methods for arriving at such evaluations.

Sometime towards the end of 1968 Amartya Sen called me to ask if I would be willing to spend a few weeks in Tehran in his stead under the auspices of the United Nations Industrial Development Organization (UNIDO), lecturing on methods of evaluation of public-sector investment projects in poor countries. He told me that in collaboration with Stephen Marglin, Thomas Weisskopf and Mrinal Datta-Choudhuri, he had prepared a set of notes for an earlier such course held in Mexico. He added that the notes were sketchy and incomplete, that he was not even sure if there was a coherent picture in them, but that this only meant I could help develop the analysis, and this would keep me from becoming bored while lecturing on the subject. He also told me that Ian Little and James Mirrlees had only recently completed a manuscript for the Organization for Economic Co-operation and Development (OECD) on the same subject (published as Little and Mirrlees, 1968), and that a number of prescriptions in their work differed substantially from those in the UNIDO notes.[4] It would be interesting, he suggested, to uncover the rationale behind their differences. He said perhaps I could look into this after having developed the UNIDO notes more fully. He warned me though that my audience would consist of people from government and industry, and this meant I would have to develop the art of iterating between theoretical imperatives and practical concerns.

I had known Sen since my childhood. Our parents had been the closest of friends for over 30 years, and he had taken a fraternal interest in my education. Since I had not been exposed to talking economics with people from the world of affairs, and as I was still raw material in economics, I could only think that Sen had approached me because (a) he thought the experience would do me good, and (b) he knew I would not let him down and not take the matter seriously were I to accept his invitation. But I could see it would involve a shift in my education and research attention, perhaps even a large shift, and this worried me. I was then a Faculty Visitor at Carnegie-Mellon University, and Richard Cyert, the Dean of the Graduate School of Industrial Administration there, had shown the exquisite sense of asking me to teach micro-economics with Michael Lovell so that I could learn the subject. With Lovell to guide me I *was* learning economics, and enjoying every bit of it. I was also being instructed in economics on almost a daily basis by Morton Kamien, Tim McGuire and

Robert Lucas. A month in Tehran would be a distraction from all this, as well as separation from my wife, and I asked her for advice.

My wife thought I should do it. She was a Quaker, and while she did not think mathematical economics was exactly a luxury, she could not help feeling I was not exerting myself in 'putting the world right', to use her phrase. In the event, this move and the research programme it entailed were to occupy my intellectual attention over the next three years.[5]

The book Marglin, Sen and I eventually wrote (Dasgupta, Marglin and Sen, 1972) differed on a wide array of matters from Little and Mirrlees (1968); so many in fact, that when a set of draft chapters of our book was circulated in the autumn of 1969, the one question I was asked by anyone who had read both was about the source of their differences. The most striking dissimilarities lay in the treatment of distributive goals (we recommended that multiple objectives be acknowledged in the choice of projects through an explicit use of weights for various social goals; Little and Mirrlees did not); the extent to which international (or border) prices should be used in project evaluation for commodities which are, or are in principle, tradable (the hallmark of the Little–Mirrlees approach was its total reliance on border prices); and in the choice of *numéraire*. Of these, the last was obviously of no analytical significance, but the first two were. On reading Little and Mirrlees (1968) I discovered the over-arching explanation behind all differences between the two approaches. It had to do with their respective presumptions about the organization of government (and the nature of governance) in poor countries. While they did not quite insist upon it, Little and Mirrlees assumed in effect that the project evaluator was part of an *optimizing government*, whereas we had supposed that the government project evaluator was someone who today would be called a *policy reformer*. We left open the motivations and abilities of the remaining branches of government, and considered in turn a number of possibilities. Our book was for this reason taxonomic, and there were far fewer sharp prescriptions in it than were to be found in the book by Little and Mirrlees. Understandably, this made it less attractive to the practitioner. The point of view we adopted saw project appraisal as an aspect of policy reform. The idea behind policy reform is to calculate the probable effects on general well-being of marginal changes in some set of control variables (for example, public investments), and then make recommendations on the direction in which the controls ought to be altered. We argued that this can usefully be done with the help of shadow prices for goods and services, the art consisting of trying to estimate them from features of the economy as it currently is and is expected to be.[6] By contrast, the Little–Mirrlees approach envisaged something like full-blooded optimization, the exercise in turn yielding an entire set of shadow prices, which could be used in principle for implementing the optimal plan. The right projects would then be those which go to fill the optimal plan.

Today, ours is a respectable way of doing public economics (see, for example, Boadway, 1978; Dinwiddy and Teal, 1987; Ahmad and Stern, 1990, among many others), but it was not then. Towards the end of 1969 public economics was enjoying a new lease of life from the manuscript versions of two great works (Diamond and Mirrlees, 1971; Mirrlees, 1972) on optimal taxation and public production, and few academic economists working on the subject at that time were prepared to take a 'reform-minded' approach to project evaluation seriously.

In the event, some took the approach as an affront. The reception among Britain's emerging public development economists to an article I wrote explaining these differences and commending our approach (Dasgupta, 1970; extended and republished subsequently as Dasgupta, 1972) was not merely chilly, it was hostile. I had offended followers of the Little–Mirrlees approach in Oxford and Cambridge, one of whom informed me that I had not understood the import of the new public economics, it was too deep for me. Several economists were engaged at that time in applying the Little–Mirrlees formulation to actual investment projects. In the process some had become true believers. It was silly of me not to have been sensitive to this. Had I been so I would have written the first draft of my article in a tone more placid than the one I had adopted. As it happened, I was a true believer of neither book. To me both were intellectual exercises. I just thought ours was the more comprehensive book (it covered a wider terrain, and had a richer sense of an economy's complexities), and only for this reason the more interesting book; nothing more. I had no idea the Little–Mirrlees approach was going to be used widely in project evaluation, so I could not make out what the fuss was all about. But fuss there was, and for a few weeks I even thought of withdrawing the paper. This would have been pointless, because the damage had been done. What I did not know then was that it was going to prove very costly for me. As was becoming my custom, I consulted my wife on the matter. She found cults merely tiresome and told me to get on with it and pay no attention. I did, but I have tried to avoid controversy in my writings ever since.

7. Trade, taxation and public production

For me the one good thing which came out of all this was an urge to dig deeper and test the robustness of Little and Mirrlees's border-pricing rule for tradable goods. My wife and I had returned to Cambridge for the academic year 1969–70. Overall it was a mistake to have done so, but we had little choice. In only one respect it turned out to be a pleasurable year for me. And that respect was Joseph Stiglitz, who was visiting from Yale.

When I mentioned to Stiglitz my research plans for the year, he said he too had been thinking of similar matters, and intended to study optimum taxation and the production of public goods under a variety of plausible 'second-best' circumstances. He suggested it would be enjoyable working together and pooling our motivations. Our long-established collaboration started there.

Much will be written of Stiglitz in the years to come. His co-authors alone would make a gigantic, well-balanced department of economics. If he looms large in economics now, he loomed large even then. He breathed economics, and as I got to know him better during that year I could not but think he dreamt economics as well. Each day he would speak to me of a new result he had discovered on some quite unexpected subject. Whenever I asked him a question in economics he responded by citing theorems from papers he had recently written. Allied to this and an astonishing economics intuition was another trait: he talked economics with anyone he met. In this he was a true Republican. Over the years this most endearing of Stiglitz's virtues has changed in complexion. In earlier days he talked of his own work as much as he listened to others. Today he does most of the listening. But this does not reflect mere politeness. It is well known among economists that Stiglitz's parents overlooked to teach him to read. So listening to others is his only way of learning. And he chooses to learn from anyone he meets, whether it be a fellow economist, or an entrepreneur, or a taxi driver in a foreign country. It has been said of Stiglitz that he can construct a theory of any social phenomenon. This is true. I have known him to produce a model of spatial competition on the basis of a conversation which he had with a cigarette vendor in New Delhi where I acted as interpreter.

There were four sets of questions Stiglitz and I explored in our work in the pure theory of public economics. The first concerned the robustness of the central result in Diamond and Mirrlees (1971), namely the desirability of economy-wide production efficiency under a set of circumstances weaker than is assumed in the second fundamental theorem of welfare economics. Diamond and Mirrlees had made three assumptions in proving their result. They were (i) that the economy is Walrasian, (ii) that while the government is capable of imposing optimal taxes on all commodities (including household income), it is unable to impose optimal lump-sum transfers among households, and (iii) that private firms face constant returns to scale in production. We showed (Dasgupta and Stiglitz, 1972) that when private producers face decreasing returns to scale (due, say, to firm-specific factors of production), economy-wide production efficiency is desirable at the second-best optimum if the government is able in addition to impose optimal firm-specific profit taxes and subsidies.[7] Our paper, and the accompanying piece by Mirrlees, showed that this was about as far as the main Diamond–Mirrlees result would go. If any further restriction were added on the powers of the government, economy-wide production efficiency would cease to be optimal.

The central set of concerns of our research programme, however, was to relax (ii) and obtain rules for commodity taxes, public-sector shadow prices, and the supply of public goods under a variety of second-best circumstances. Towards this we studied a closed economy in one paper (Stiglitz and Dasgupta, 1971), and a trading economy in another (Dasgupta and Stiglitz, 1974). It was, of course, the concerns of the latter paper which had drawn me to this research programme in the first place, for I had wanted to test the robustness of the Little–Mirrlees border-pricing rule. I knew Little and Mirrlees did not need for its justification Diamond and Mirrlees. (The border-pricing rule was valid under more general circumstances, pertaining as it did only to a part of the economy-wide production sector.) In addition to identifying the weakest set of circumstances under which the relative shadow prices of two tradable goods equal their relative border prices (see also Diamond and Mirrlees, 1976), Stiglitz and I obtained optimal second-best commodity taxes and tariffs under various circumstances.

This article was written during the academic year of 1970–1, when Stiglitz had returned to Yale. I had known in advance that I was going to be unemployed that year, and so had requested Sen to invite me to the Delhi School of Economics. This he did with his customary efficiency and concern. I had thought collaboration at a distance on a technical paper would prove difficult. In the event it did not. Stiglitz and I had worked so closely the previous year that we were by then able to read each other's mind.

Neither of our two articles on the structure of indirect taxes and tariffs makes for pleasant reading. Inevitably for this kind of investigation, there were a great many computations to get through, and several of them we got wrong. But the latter paper, bringing about as it did a merger between the theory of trade policy and the new theory of public finance, had some merit. I infer this from the subsequent work by Findlay and Wellicz (1976) and Srinivasan and Bhagwati (1978), among others, which explored a number of ideas very close to ours.

8. Exhaustible resources

By the beginning of 1972 I was again at a loose end. I had become bored with the new public economics as it was then being practised. I understood the technical core of the area and did not now wish to undertake any more computations on the structure of optimal taxes, or of public-sector shadow prices. It was a feeling all of us, I am sure, have from time to time; a feeling of staleness. I had spent too much time operating on the differential calculus during the previous two years, and had run out of ideas. In any case, I felt my work here had been derivative. Diamond and Mirrlees had not merely rejuvenated public finance, they had routinized it as well. Fortunately, I had been given a teaching post at the LSE in the autumn of 1971. This meant I did not any longer have to look for a job, nor ask each morning: 'What do I do today?'

Escape came from a conversation I had with Geoffrey Heal in Cambridge. We both had recently read Jay Forrester's simulations of the world economy grinding to doom through the depletion of environmental resources (Forrester, 1971). As had others, we too had noticed an absence of economics in Forrester's world. But if Forrester's models did not have human agency in them, growth models and economics texts were no better; they did not have depletable resources in them. Heal and I asked each other what we knew of the economics of exhaustible resources, and concluded we knew nothing.

In today's academic world it is difficult to recapture yesterday's intellectual innocence. Every graduate student in economics today knows the structure of competitive price trajectories of exhaustible natural resources under rational expectations. At the time I am speaking of, even Solow probably did not figure it out without putting pen to paper. My guess is at least 500 economists could have written my first paper with Heal on the subject (Dasgupta and Heal, 1974). That at most three or four economic theorists thought the problem worth exploring at that time, no more and not earlier, remains a mystery to me.

I had known Heal casually over a number of years. We had been contemporaries in economics at Cambridge. He was already famous for his work on decentralized planning mechanisms, and had just published a book which continues to be the standard work on the subject. He was managing editor of the prestigious *Review of Economic Studies*, and was about to be elected to a Chair at Sussex at an unusually young age. I knew of the clarity and brilliance of his mind; what I did not know then was its generosity and sensitivity. During our long collaboration in the 1970s he became, with Stiglitz and Eric Maskin, one of my closest friends in the UK.

As we were both products of the growth-modelling years, it did not occur to us to study the economics of exhaustible resources in the context of a model of a single extractive industry. This was fortunate. Had we done so we would have merely rediscovered Hotelling (1931), a paper we were unaware of (and had no reason to be aware of) then. Instead, we leapt straight into a complete intertemporal general equilibrium model.

Our idea was to use the Ramsey framework for exploring optimal depletion and capital accumulation policies in a closed economy. Pretty much the first result we obtained was the one which is now called the Hotelling rule, that along an optimal path the present-value price of the resource remains constant. However, the bird we were trying to catch was harder to locate. We wanted to know something about intertemporal consumption possibilities in a world with depletable resources. We began conservatively and assumed away technological progress. It made sense then to consider a stationary population profile. So we did this. It was only a matter of time before we realized that whether consumption can be kept bounded away from zero (what these days would be called 'sustainable consumption') depended on the asymptotic value of the elasticity of substitution between fixed capital and the flow of resource in production as the rate of flow goes to zero. Quite obviously, all is well if this elasticity is greater than unity (the isoquants cut the capital axis in this case). A slightly involved argument showed that the economy is doomed if this elasticity is less than unity (the economy is in effect a cake-eating society in this case). But we could not work out the telling, razor-edge case when it equals unity. In short, we could not trace consumption possibilities in a Cobb–Douglas world. All we could show was that if future utilities are discounted at a positive rate the optimal thing to do is to allow consumption to go to zero. This did not tell us if consumption is unsustainable in such a world. Nor could we work out what an optimal programme looks like if utility is not discounted.[8]

Prototypical examples of exhaustible resources are fossil fuels. We knew that for energy resources an asymptotic elasticity of substitution which was less than unity was the right one to work with. Casual empiricism suggested this. But there was nothing interesting to discuss about such an economy unless we allowed for increases in our ability to draw upon new sources of energy. At that time there was much talk of the possibility of obtaining energy from nuclear fusion. This was years away though, and it all looked (and it still looks) uncertain. Heal and I decided to move in a different direction from the prevailing vogue of modelling technological change in a continuous, factor-augmenting fashion, and ask what today's and tomorrow's extraction policy ought to be if we are currently trapped in what amounts to a 'cake-eating' situation, but where at the same time there is a prospect of our being able to harness an enduring source of energy at some uncertain future date. (Nordhaus subsequently dubbed this a 'backstop technology'; see Nordhaus, 1973.) We showed that the right thing to do is to pretend we will never be successful at making the innovation, and to change the instantaneous social welfare function in a way that correctly takes into account that this is merely a pretence. (In some extreme situations this transformation amounts to increasing the rate of discount at each instant by the probability rate that the backstop technology will arrive at that instant conditional on its not having arrived until then; that is, the 'hazard rate'.) As it is deterministic, this modified problem is easier to solve. But the two problems have the same solution. While this was one of the central results of the paper, our over-arching motivation at this point was to study the optimal behaviour of a society which hopes to make a transition from an exhaustible resource base to a source which is enduring.[9]

Today I have the impression that this paper of ours is thought well of by the economics profession. At that time, though, I had no reason to think this would be so. Certainly, there was no immediate response from colleagues in the UK which would have suggested otherwise. This is a problem we have to live with in economics. It is an easy matter to assess whether a problem is an important one; it is very hard to know how to go about a problem we know to be important. No one can be certain, and the gap between intention and execution can as a result be very wide indeed. At that stage of my own development I had, perhaps, a greater need for reassurance of the value of my work than my contemporaries did of their work; but the fact remains, we all need reassurance constantly that we are not wasting our time. A letter Solow wrote to Heal and me on reading our typescript contained the sort of fraternal academic sentiment which is capable of changing a young researcher's sense of his own worth and of the niche he might seek to occupy within the international community of scholars. Solow wrote it as a matter of course. The gesture was an instance of his sense of academic responsibility, but it also reflected the characteristic Solovian impulse I came to know later. For me, it was a most important letter. It was the first letter of detached approval I had received from one of my intellectual gods. It gave me a feeling of confidence I badly needed then.

The suggestion that Heal and I write a book on the economics of exhaustible resources (Dasgupta and Heal, 1979) came from Frank Hahn, who was then editor of the Cambridge Economic Handbooks. Our instruction was to write a monograph on the subject, and Heal and I separately prepared a pair of survey articles which together were to be the model for what the book would be (Dasgupta, 1974a; Heal, 1975). In the event the planned monograph became a treatise, and it took us four years to complete it. One reason things went out of control was that I became somewhat obsessed with the subject, and Heal was too generous not to indulge me. I had the idea that all of micro-economics could be developed round the theme of exhaustible resources. It seemed to me it would make the teaching of the subject less dry, and at once more urgent. So we tried doing this. But this was only one reason the book became a treatise. The other, although I did not realize it at the time, was the influence of Paul David.

My wife and I and our newly born daughter spent the academic year of 1974–5 at Stanford University. Stiglitz was moving there then and had arranged the invitation in advance. At that time I knew no economic history and no economic historians, and imagined they must all be engaged in the mining of records, with little understanding of theory. At Stanford I met Paul David, and he shook me out of this comfortable illusion. Not only was he a master of history, I realized he was a brilliant theorist as well, and was using it in a most innovative fashion to interpret the course of technological innovations.

I began work on my book with Heal that autumn and chose first what promised to be an easy chapter, on externalities. I read Scott Gordon's paper on the problem of the commons (Gordon, 1954), and reformulated it as a symmetric game involving *N* users. This meant there was no free entry to the commons. In searching for collective solutions it struck me that monitoring non-compliance should be relatively easy in cases where the commons are geographically localized. This meant free-riding must be minimal for such commons.[10] I drafted a section along these lines and described the model to David. My thought was that this would impress him. I do not know if it did; what I do know is that he was already familiar with the notion. He remarked that a property being communally owned did not mean people had free access to it, and said historically local common property resources had often been managed quite efficiently. He also seemed to relish the opportunity of telling me that misunderstanding about the commons was common among non-historians.

I took to engaging David in conversation in the lobby of the fourth floor of Encina Hall (which houses the economics department at Stanford) to tell him the things I was thinking of. He gave me the kind of quality time an academic today has no business to expect from another, and I discovered that he found every theoretical result a potentially interesting one. Whenever I told him how I had formulated a problem he would take a deep breath and go into the kind of slow, long monologue for which he is now famous. In our earlier encounters the effect was to make me think this was the historian in him. I soon recognized it as a ploy for buying time. What he was doing was talking grammatically correct, inconsequential sentences to begin with so as to absorb what I had told him and to think simultaneously of ways of improving upon the formulation. This he usually managed to do. He would search for either historical or current approximation in whatever was put to him, and I began to feel this had to be the acid test for anything Heal and I wrote in our book.

This was not constraining. Rather, it freed us to explore a number of questions that pure theory *qua* pure theory would have found less than interesting. So, for example, we sketched how net national product ought to be measured in an economy with exhaustible resources, investigated the effect of various types of taxes on the pattern of their depletion, explored alternative ways of using a common property collectively, developed cost–benefit rules for government research and development in alternative sources of energy, discussed various formulations of the idea of keeping future options open, exposed the possible multiplicity of pollution-tax equilibria, and so on. All this made the book long, and it took us a while writing it. The book was not published until the end of 1979.

9. Technological change and market structure

My interest in the economics of technological change began with my first article with Heal. Our paper took the stochastic renewal process underlying the discovery of a backstop technology to be exogenously given. The thing which needed doing next was the modelling of the research and development (R&D) process itself, the idea being to make the probability distribution of the date of discovery in a planned economy a policy variable. This was done in Dasgupta, Heal and Majumdar (1977; see also Dasgupta, 1982a). However, what intrigued me more was R&D activity in a decentralized economy. I knew even then that patents and secrecy are ways private firms try to earn profits from their R&D expenditures, profits they need to expect if they are to enter races at all. It occurred to me that R&D competition is different from the competition then regarded as implicit in the Arrow Debreu model. So it called for investigation.

I read Schumpeter on the dynamic advantages of monopoly (Schumpeter, 1934), and Scherer on industrial structure and R&D (Scherer, 1970). They were both revelatory, in many ways outstanding, but they reflected styles of research I was beginning to find irksome. Whereas the first conveyed to me only a vision with no formal argument, the latter struck me as consisting of case-studies and cross-sectional regularities without the backing of theory. I began to feel economics had become far too compartmentalized even as regards what was acceptable as analysis. If you studied a competitive industry you had the full force of an enormous technical apparatus to help you; and you would be expected to take advantage of it. But if you studied R&D competition you were allowed to wave your hands. What irked me even more as I read further of the applied literature on technological change was that otherwise able economists wrote as though they were oblivious of the fact that there was no theory to guide them.

While at Stanford I mentioned this to Stiglitz who, needless to say, had read neither work, but appeared to know both. He felt we should start our work in the area not with R&D competition, but with oligopolistic competition among owners of an exhaustible resource and firms with access to a backstop technology. He was right of course. At that time we two understood the economics of exhaustible resources better than most people. We knew it was the simplest context in which one could develop a formal language for dynamic oligopoly. We had the necessary tools at our disposal to offer an account of competition for the supply of a service between those whose production costs are low but whose stocks are finite, and those who are capable of producing unlimited amounts but at a relatively high cost per unit produced. In short, we wanted to explore the way the transition from an exhaustible resource base to a durable source would be affected under various kinds of market structures. This we did in Dasgupta and Stiglitz (1981) and Stiglitz and Dasgupta (1982).

It was only after Stiglitz had moved to Oxford in 1976 that we began to work on the phenomenon I was itching to understand, namely, 'Schumpeterian competition'. Solow had invited us to prepare a manuscript on technological change for the 1977 International Economic Association World Congress in Tokyo, and this concentrated our joint attention.[11]

It is not easy to bring back today the kinds of argument we were used to in talking of competition in the mid-1970s. Game theory had yet to make a dent in the way the typical economist reasoned. Several years earlier I had read Luce and Raiffa's book on game theory, and had liked their style of analysing social problems. I rather doubt if Stiglitz knew any game theory then, but I knew that he knew Cournot's and Bertrand's models, possibly also two-person constant-sum games. Not that it mattered. He could think like a game theorist when he was required to, and I pressed him to so think. He did not like game-theoretic expressions though. When I drafted something and used terms like 'continuous strategy spaces', or 'Nash equilibrium', he would ask, 'Do game theorists really *talk* like that, or are you having me on?'

Our aim was to *explain* market structure. We wanted to write down formal models in which the structure of industry, R&D activity, and the pace and timing of innovations would be jointly determined by such matters as demand conditions, R&D technology, the rate at which firms discount profits, and so forth. Our basic model, of R&D and output competition among firms involved in process innovations (Dasgupta and Stiglitz, 1980a), was designed to make analytical sense of a number of stylized facts codified by applied economists.[12] It was a model of monopolistic competition. In it, competing firms discover for themselves niches not only in the product space, but also in the space of R&D activities.

In a parallel paper (Dasgupta and Stiglitz, 1980b) we explored a sharper problem, one of patent races. It struck us that the phenomenon of pre-emptive patenting, discussed informally by Schumpeter, makes most sense when firms face correlated risks in their R&D strategies, and we formulated this idea. But we wanted also to develop an intertemporal account of competitive patent races, where a number of firms (the number being determined by the analysis) enter a race, even though they know only one of them will ultimately be successful. This became the central construct of our paper.[13] To have a manageable problem we assumed R&D risks to be independent of one another. We showed that free entry leads to too many firms doing R&D. In particular, we showed that competition leads to an excessive total expenditure on R&D.

Sometime later, in collaboration with Eric Maskin, I studied a different question, whether competition for a patent (or priority) leads firms (or academic scientists) to choose overly correlated risks in their R&D projects (see Dasgupta and Maskin, 1987). To have a manageable problem we assumed the number of competitors to be given. We discovered that in plausible circumstances competition leads to the choice of R&D projects which are more correlated than would be desirable were collective efficiency to be the goal. In short, private corporations and academic scientists are not lone wolves trying to differentiate themselves from their rivals as much as possible. They and their rivals work on research projects which are overly similar.

In recent years the analysis of patent races has come to dominate theoretical work on the micro-economics of technological change. This is in part due to the habit American theorists have of reading only American journals and books. I have not met many economists in the USA who have read the first of my two papers with Stiglitz on technological change, even though its contents have entered UK texts. From citations, I infer they have heard of the second. But the first is much deeper, and has the kind of features macro-economic models of the long run are only now beginning to incorporate (see also Dasgupta and Stiglitz, 1982, 1988).

10. The economics of science

The work Paul David and I have done (Dasgupta and David, 1987, 1991) on the economics of science and technology began towards the end of my second extended visit (1983-4) to Stanford. David felt industrial organization theory had been exploring technology at the expense of science. He suggested we explore the institution of science as one possible solution to the resource allocation problem associated with the production, dissemination and use of knowledge. Traditionally, science and technology have been distinguished by the nature of their products; for example, abstract ideas as against concrete objects and marketable formulae. Our analytical training led us to think that product characteristics ought not to be used as the basis for institutional classification. We felt, rather, that differences in the characteristics of products should be traced to differences in the institutions producing them. As a resource allocation mechanism, the republic of science stands in marked contrast with the institution of technology, in that the latter views knowledge as an economic commodity, and treats it so. Norms of disclosure, the system of rewards to discoverers (e.g. patents, and the rule of priority) and, more generally, the social ethos, are very different in the two institutions. We showed that these differences have only something to do with the fact that their generic end-product, knowledge, is both durable and jointly usable. (These two characteristics of knowledge are the objects on which a good deal of the sociology of science is based.) We demonstrated that they have much to do with the inherent difficulties principals (i.e. patrons, sponsors, governments and private corporations) face in monitoring researchers' zeal and in identifying their talents. In short, the production of knowledge is shot through with uncertainty and asymmetric information, both about the effort researchers put into their work and about researchers' innate skills. We argued that the economics of science must start from this observation. The institution of science has traditionally been the purview of sociologists. Our intention was to ground it on modern resource allocation theory.

The approach we have taken on these matters has allowed us to make contact with a wide variety of features one observes in the institutions of science and technology. We are now in the process of investigating more fully the theory's implications for policy.[14] Moreover, our argument has in the main been analytical so far. The beginnings of a historical study of the growth of the institution of science as seen through our theory has been made in David (1988). We are now in the process of integrating our ideas in the form of a monograph on the economics of science.

11. Roots

My lack of grounding in economics forced me in my early years as an economist to read nothing outside the subject. I needed to catch up. This made me narrow in my intellectual outlook and interest. I began to find myself dull, and wondered if my wife found me so as well. In particular, it saddened me that I had lost touch with philosophy, a subject I had been very fond of in my undergraduate years at Cambridge.

Cambridge in the early 1960s did not take either moral or political philosophy seriously. For my part, I had found the prevailing books on moral philosophy parched of life, and had stopped reading them. But political philosophy was another matter. That it had recently been pronounced dead by experts only meant there was nothing contemporary for me to read; it did not mean I could not reflect on the good society in the modern world. The problem was, I had no intellectual machinery with which to reflect. This had stalled me. It was not until recently that I arrived at an articulated political philosophy for myself. But by instinct and upbringing I was even then what one would label a left-liberal in the USA, and a liberal democrat in England. In this, as in many other things, I had been profoundly influenced by my father.

The line of Dasguptas I belong to comes from Gaila, a village in the district of Barisal in what is now Bangladesh. As far as the records show, we had been there since the middle of the seventeenth century. My father was born and raised there. Ours was a lineage of scribes, and therefore of somewhat poor means. Along the way a few had attained a measure of scholarly eminence, but when my father was born our line was financially impoverished. In his early years, as I understand it, my father displayed no exceptional intellectual talents, and no special attention was given to him by his teachers. There was no patron to assist him when he sought higher education, and he pursued it by means of token scholarships and massive loans, and encouragement from his eldest brother. I understand from him that the only thing he ever consciously desired was a university teaching post. That he even entertained this thought, let alone managed to lift himself out of what was a totally unpromising rural background to become the founder of modern economics in India, reflects a commitment to learning I have seen in no one else. We were close, and even as a schoolboy I thought he was a wholly admirable person.

My father's academic life collapsed in 1946 when we had to leave Dhaka. From then until his retirement he led a roving life, from post to senior post, none of which held much attraction for him, because he never again found the students he needed and deserved. After 1947, which is as far back as I am able to recollect when looking into my past, my parents and my sister and I spent only three years together. This was in Washington, DC, during 1950–3, when my father worked for the International Monetary Fund (IMF), on leave from his professorship at Banaras Hindu University in what is today Varanasi. My sister and I loved our schools in Washington and my mother enjoyed the freedom which financial security and an enticing city can together offer. I do not think my father enjoyed it much though. He missed lecturing, and tried without success to convert his office at the IMF into a classroom. My fondness for the USA dates back to this period. It is because of my childhood in Washington that I find myself perfectly at home whenever I am in the USA.

Since we had no real rooting, I was sent to boarding school on our return to India. This meant my parents and I talked only during the holidays. At the time I got to know my father properly, which was round about 1956, he used to devote his intellectual energy entirely to economics. Because he never talked of it, I did not know then that he had a deep knowledge of Sanskrit and Bengali literature. I had just entered my teens then and had begun emulating my sister by reading late-nineteenth- and early-twentieth-century novels and plays. We found that our father had read only a bit of Shaw, not much else. This made me think he had no interest in literature. What I found him reading instead were the classics of economics, which were of no interest to me. So we talked of other things when I visited home. He had a passion for reason and honesty, and convinced me by means of examples that almost all the world's politicians were either stupid or hypocritical, or both. Among political leaders the one person he admired was Mahatma Gandhi, and he explained to me why Gandhi was admirable. Since he was not going to teach me, I had to rely on my sister to help me in my study of literature. She was quite scintillating, and because she was a bit older than me, she had time for me.

The period 1956–8 was one of the happiest and most fruitful of my life. I transferred to what was for me an ideal boarding school very near Varanasi, in Rajghat, on the confluence of the rivers Ganges and Varuna, and my sister moved home to finish college. It was the last time we were all near one another as a family, so to speak. The combination of my teachers, my parents and my sister ensured that I developed intellectually. It seems odd to me now, but I sensed even then that I was growing, and it felt good to be alive. I was not to recapture that feeling until years later when I had entered graduate studies.

While I admired my father, I knew we were temperamentally different. For example, my mother used to say that my father's notion of perfection was a world in which the only thing anyone ever did was read. She and I agreed this could not be a perfect world, because in terms of goodness it would be surpassed by one where everyone read only some of the time. In this I resembled my mother more. My mother had no interest in economics, but she had a great deal of interest in my father and in my sister and me. She was also the adopted mother of the many students at the university who, away from their home, came to our home. My sister and I had to share our mother with others, but we found it entirely natural that we should do so. She was the anchor in our lives, and but for her my father would not have been able to pursue his academic work unhampered by concerns about the way our lives were organized. We took this for granted, but even in those days I had the vague understanding that without her we would be rootless.

If my father was Socratic in his approach to life, he was also innocent of the irrational drives which influence even the most rational of persons, and of the complexities which surround both personal and social behaviour. For example, he once told me that academics must inevitably be liberals in their politics because intellectual endeavour can thrive only in a liberal environment. I imagine this is why communism and narrow nationalism held no attractions for him. He felt if only the poor in India were to cease admiring the rich and were instead to laugh at their conspicuous consumption, the rich would be embarrassed into doing something worth while with their personal wealth. My father thoroughly distrusted revolutions. I discovered that he admired the social writings of Bertrand Russell. So, of course, I read him. And I read him in great chunks. To us, Russell's attractions lay not only in the clarity of his writing, but also in an absence of humbug the Indian intellectual tradition embraces all too eagerly. Most of all, we liked his commitment to Utilitarianism as the intellectual basis of liberal democracy. The one aspect of Russell my father could neither comprehend nor approve was his habit of getting married every now and then. He felt it reflected a certain waywardness of character. I was then too young to know or to comment.

12. Rawlsian savings and population ethics

Until 1972 the only political philosopher I had read was Russell. That summer I read John Rawls's great work (Rawls, 1972) and was overwhelmed. I had never read anything with its sweep before. Moreover, it articulated my own embryonic political beliefs, and it did so with a clarity and thoroughness I would not have thought possible. There were any number of details which looked unsatisfactory to me (for example, the lexicographic ordering of the principles of justice was not persuasive), but this did not bother me. They were details, to be chewed over by others more patient than I. The overall scheme of thought appealed to me, and I wanted to check if it was coherent.

This posed an immediate problem, in that I could not make sense of the first part of Rawls's second principle, on justice among the generations. My understanding of the implications of various structures of 'preferences' for intergenerational savings was sufficiently good by now. For example, I knew that were each generation's utility a function of its own consumption and the utility of the generation succeeding it, we would be half-way down the road to Ramseyesque preferences, and there would be nothing distinctive about Rawls. So I took a different route. Following what I took to be Rawls's conception, I assumed that each generation has concern over its own consumption and the *consumption* of the generation following it. On modelling the Rawlsian contract as an intergenerational Nash equilibrium, I discovered (Dasgupta, 1974b, c) that it yields Pareto-inefficient consumption programmes.[15] This was so much at variance with the rest of the book that I expected Rawls to respond by saying I had not understood him. Instead, he replied by asking in effect: 'What do we do now? How do we rescue the principle?' I could not think of a way, because everything I tried reduced either to Utilitarianism (i.e. that each generation is formally of Ramseyesque disposition) or to a forced picture of the way intergenerational bargaining is envisaged behind Rawls's veil of ignorance.[16]

Arrow, who was visiting England in the spring of 1973, read my typescript and felt otherwise. He thought Rawls's savings principle must be intergenerational maximin.[17] This did not strike me as reflecting Rawls's text, but as I always felt mentally retarded in Arrow's presence, it did not occur to me to argue the point with him. My main problem was that I was tongue-tied whenever we met. In earlier days it had not mattered, because I could, and did, avoid him when he visited England. But on this visit things were different. He was sufficiently intrigued by the problems my paper had raised to begin putting intergenerational maximin through its paces. As we were working on similar problems, he made a point of discussing them with me on a regular basis. It was a terrifying experience, and I knew something had to be done. So I devised a scheme of nodding or shaking my head at random intervals whenever he spoke to me. This gave him the impression I was following him closely, and that perhaps on occasion (when I shook my head) he was being dense, maybe even saying something wrong. There must have been the chance moment when he did say something not so penetrating and I had simultaneously shaken my head, because I sensed that he regarded me well.

We agreed that intergenerational maximin does not pose a novel problem if generations are Ramseyesque, since the framework is then formally the same as Utilitarianism.[18] So Arrow worked out (Arrow, 1973) the maximin savings policy when generations have the sort of (partially truncated) preferences I had assumed in my paper.[19] His solution was ingenious, but in highly productive economies it was intergenerationally inconsistent (Dasgupta, 1974b, c), and I felt 'contracting' generations would not abide by this. So we were back to square one.[20]

But this was only the beginning of my worries on matters philosophical. In trying to think through Rawls's contractual theory of justice I began to have doubts about Classical Utilitarianism as a basis for population policies and, by extension, reservations about the philosophical foundations of my early paper on optimum population. I also could not see how contractual arguments alone could be used towards this end. Being secular, I could not think *potential* people could count in the same way as *actual* and *future* people should be counted. It seemed to me that God faced an easier problem than the one we are confronted with as prospective parents. In the Genesis problem there were no actual people - all persons were potential. In such a world God could consistently be a Classical Utilitarian. But in our world there are actual people. It is an obscene ethics which asks parents to practise 'impartiality' across their children and their potential children.[21]

I discussed these matters with the philosopher Simon Blackburn, who was a friend from undergraduate days, and we prepared a draft manuscript in 1973 containing a sketch of an ethical theory which encompassed both population and intergenerational savings in an actual world. The idea we pursued was that each generation cares about its own living standard and for the next generation's size and its living standard. (In this we followed the motivational assumption of my papers on Rawlsian savings.) The theory distinguished actual and future people from potential persons. It identified Nash equilibrium population and savings rules as the basis on which policies ought to be designed.

Alas, Blackburn soon had doubts about our formulation, so joint publication was no longer an option. While the theory had difficulties, it seemed to me worth an air. I wrote a technical version for publication (Dasgupta, 1974d). As there has been little interest in the problem, I have felt no pressure to work at it intensively. But I have returned to it whenever refinements have occurred to me (see Dasgupta, 1988a, 1989c, 1991a). One of the theory's features is that it does not harbour any of the litany of paradoxes Derek Parfit and his followers have unearthed in their personal reading of the Genesis problem. There are other attractions as well, chiefly that it is not mired in the Genesis problem, and instead addresses actual problems. It is just possible that in my latest version I have got the theory right.[22]

13. Institution design

I read Rawls's book several times over the next few years to get a feel for philosophical thinking. It taught me to deliberate ethical questions in a professional way. I practised my growing confidence in this on the population problem. That I was able to cast it in a way which required little economics may have then been one of its attractions, for I kept my philosophical thinking on the good and just society separate from my economics thinking. The fact is, I would not have even known how to connect them were I to have in those days thought about the organizational structure of a just society. I had only a vague feeling of dissatisfaction, of not being able to make a connection beyond the confines of the two fundamental theorems of welfare economics; and this I realized was not much of a connection. My uneasiness was no doubt traceable to Rawls's own, understandably inexpert, treatment of such matters. He was strong on decision theory (his emphasis on rules over discretion proved that), but he was weak on organization theory.

In the summer of 1977 I read Arrow's *Limits of Organization*, and it changed my way of doing economics. I do not know if many people have read this slim volume (Arrow, 1974); there are not many references to it in the economics literature. But for me it remains one of the great masterpieces in the social sciences. Even the opening sentences hit me in the eyes. Arrow began by quoting the musings of the sage Rabbi Hillel: 'If I am not for myself, then who is for me? And if I am not for others, then who am I? And if not now, when?' Arrow then went on to show how in addressing these questions we are led inexorably to an inquiry into the organization of society, and thereby to a resource allocation problem. He noted that markets are a form of organization, but that other forms of organization develop and are needed to develop when markets cannot function because of transaction costs and informational constraints.

Arrow's approach to these questions was bewitching to me. At one moment he was talking of the internal organization of firms, at the next of the organization underlying societies. He found it plausible, almost natural, to switch back and forth between the SMALL (on which we had theorems) and the BIG (which we understood little), and he did this without strain. The book reflected a kind of intellectual voyage I had not encountered anywhere else. I felt he must have taken great risks in even thinking he could write a book like that. I thought perhaps one day I too would write such a book (perhaps even by combining and extending his and Rawls's methods), not on political philosophy, of course, nor on the theory of organization, but on political economy, based on both analysis and evidence; a sort of grand synthesis of the social world, of the circumstances in which we are born, and the way we live and die, all seen through the spectacles of modern resource allocation theory. There was one snag though. Arrow stopped short of an analysis of informational asymmetries. And even though I had read Spence (1974) and Rothschild and Stiglitz (1976), I had no feeling for the problem of organization design under asymmetric information. In this, as in so many other things, I was yet to be tutored by Eric Maskin.

I had met Maskin the previous year. He had come from Harvard as a 'post-doc' to work with Hahn at Cambridge, and had been asked by Arrow to get in touch with me. In the event, he did not. Instead, I got in touch with him. At that time I was trying to make sense of the seemingly paradoxical results in Rothschild and Stiglitz's (1976) model of a market for insurance, that market equilibria under circumstances of asymmetric (and endogenous) information may not exist. Along with many others I had recognized this to be a seminal piece of work. We knew that while cast as a simple example, it was trying to tell us an important truth. The problem for me was that I could not fathom it. Whenever I asked anyone, including the authors, why the model does not have an equilibrium under a wide class of circumstances, all I got by way of an answer was a repetition of the argument deployed by Rothschild and Stiglitz in showing that there is no equilibrium. For a while I did not know why such an answer was unsatisfactory to me, but unsatisfactory it was. It took me some time to realize that what I was seeking was a game-theoretic explanation. I felt that while Rothschild and Stiglitz had not cast the model of an insurance market as a game, it ought to be modelled as one. This way we would be able to appeal to equilibrium existence theorems and understand why under plausible circumstances the model does not possess an equilibrium. A few days after our first conversation Maskin came to my office at the LSE with a partial formulation. It took us some time to realize that pay-offs in the Rothschild–Stiglitz game are discontinuous, that the model is formally akin to Bertrand competition. We now knew the source of the problem; it was the explanation I had been seeking. So we decided to abandon the Rothschild–Stiglitz model for a while and to study instead games with discontinuous pay-off functions. Thus began our decade-long collaboration in proving a set of (positive) theorems on the existence of Nash equilibrium in mixed strategies in discontinuous games, which we applied to show that each of the then existing economic models (including Rothschild and Stiglitz, 1976) which did not have an equilibrium in pure strategies possessed equilibria in mixed strategies (Dasgupta and Maskin, 1986a, b).[23]

Hahn had informed me that Maskin was about to bestride social choice theory like a colossus. He was to prove right of course. But colossus or not, Maskin soon fulfilled my image of an inspired intellect of the purest kind. He was underbearing in his manners, but when standing by a blackboard with chalk in hand he always grew in confidence, and seemed to grow in size as well. As he was in the process of creating a combined framework for studying incentives and social choice, he had, of course, a lot to be confident *about*. However, I was innocent of all this, and he tutored me over the many weekends he visited my wife and me at our home in London.

Our work sessions were interspersed with moments of the infantile humour both he and I are susceptible to. For example, we found American commercials both funny and ridiculous. So, mimicking the voice in a commercial I might say to him when we had formulated a theorem: 'This is the best theorem money can buy. At the price we are charging it is bigger and better than all other theorems.' To which he would respond by mimicking some other voice: 'Where I come from, mister, bigger *is* better!' Or I might interrupt our proceedings and say: 'This theorem is so pure it is whiter than white!'; to which he would respond: 'The next theorem is going to be so pure, it will be bluer than white!' And so on. We knew this was faintly ridiculous, but there it was. We would also stop work to cook gourmet food with my wife, go to films, and listen to him teach our daughter to play Cole Porter on the piano. I recollect this because I cannot think how else we could have taken 10 years to publish our papers on discontinuous games.

When we had spare time, we discussed the theory of mechanism design, or planning under asymmetric information. Our joint paper with Peter Hammond (Dasgupta, Hammond and Maskin, 1979) was an attempt to codify, and in parts extend, the theory as it then existed. These conversations, and work on our three-way article, provided me with a glimpse of the way certain classical issues in political philosophy, such as for example the worth of individual rights, could be approached through modern resource allocation theory. In my Inaugural Lecture at the LSE (Dasgupta, 1980) I offered an instrumental basis for individual rights in a world with asymmetric information, and used the resulting framework for comparing the philosophical positions of Rawls, Robert Nozick and Fredrick von Hayek. I had it in mind as the chapter which Arrow never wrote in his *Limits of Organization*. Today I like to think it can stand on its own.[24]

14. Environmental resources

Nepotism has its merits. Even as I was completing my book with Heal, Edward Dommen of United Nations Conference (UNCTAD) approached me to write a monograph on social cost-benefit analysis of investment projects with environmental effects for a joint UNCTAD UNEP (United Nations Environment Programme) research programme on environment and development. He wanted me to write the chapter which never got written in either Little and Mirrlees (1968) or in my book with Marglin and Sen.

This was 1978. When one reflects on the extent of professional consciousness on the state of the environment in poor countries at that time, one can but wonder why United Nations agencies are routinely thought to be intellectually timid. That I was Dommen's wife's brother-in-law goes to explain in part why he approached me, but only in part. I was one of only two economists who would have been able and willing to write such a book then. The other was Karl-Göran Mäler. But neither Dommen nor I knew Mäler then.

Heal and I had sketched the economics of renewable natural resources in one of the chapters of our book. It was not until I agreed to write the monograph for Dommen that I realized environmental pollution problems have the same analytical features as do problems associated with the management of fisheries, ground-water, forests and soil quality. If instead of studying the pollutants we were to study the sink into which they are deposited, we would have a problem very much like those to be found in the economics of renewable natural resources. Insights from the study of any one of these resources could be used for understanding problems posed by another. I could see that ecologists (for example, Ehrlich, Ehrlich and Holdren, 1977) would agree with this viewpoint, but they had not articulated it. Their training did not encourage them to seek this sort of formal connection. I could also see that ecologists were streets ahead of economists in their awareness of the magnitude of environmental problems in poor countries. The manuscript I prepared, at the end of 1979 (see Dasgupta, 1982c), put these thoughts together into a unified framework for studying the welfare economics of environmental resources in poor countries.

At UNEP, Yusuf-J. Ahmad was in charge of the programme for which I was writing the book. In the autumn of 1981 he organized a meeting in Istanbul for civil servants from developing countries to discuss my final manuscript. Some 25 senior civil servants participated. As I recall it, all insisted that while they had enjoyed reading the book (a clear lie, none of them had read any of it) it was not relevant for their countries, which were after all very poor. They said only rich nations could afford to worry about such matters. They said coming as they did from Third World countries they were interested only in economic development, not in the environment.

It was something of an experience listening to this drivel. I thought perhaps they had been instructed in this fashion at some Third World Summit, the sameness of their language was uncanny. But there was not much I could do about it. At that time there were few rural case-studies to appeal to for arguing that even if the problems analysed in my monograph appeared alien to them, they were very real to the rural poor.[25]

In the event, their view proved to be representative. The book bombed. Development economists could not care less about environmental matters, and environmental economists in Western industrial democracies could not get excited about the special problems poor countries are prone to. I had the strong impression that economists in general did not find environmental matters of any moment, for my book only got a mention, not a review, in the *Economic*

Journal. That the monograph was not about environmental amenities, but about the basis of all production in any country, did not seem to matter. I tried not to feel depressed about it, and found that the only way to fend off depression was to work on something else, which in this case was my old subject, the normative basis for population policies. It was Karl-Göran Mäler who told me to take no notice and to continue working in the area of environment and development.

I had first met Mäler in the spring of 1979 at Nairobi airport. We were attending a meeting at UNEP; and about the first thing we did was to hire a taxi to take us to view giraffes in the nearby game-park. He was at that time nowhere as famous as he is now, but I had read his work. Several years earlier he had formulated a definitive mathematical account of general equilibrium in an economy with environmental by-products, using in part the then newly devised materials-balance conditions. While abstract, his book was operationally motivated, and among other things he discovered conditions (viz. weak complementarity) under which we are able to use market data to measure the benefits accruing from non-marketed commodities (Mäler, 1974). This book remains the outstanding treatment of the subject. But at the time I met him, I rather doubt if many had read his book. It too had not been reviewed widely.

Mäler's commitment to bringing environmental problems within the fold of modern resource allocation theory has been singular. He has produced students, published both analytical and applied papers of exceptional quality, and has been a central figure in the establishment of a European base for the subject. For a brief period our work did not bring us together, because my own interest continued to lie at the interface of poverty and the environmental resource base. But we had a long-term project in mind, and it took us seven years to realize it.

Our experience at the Istanbul meeting had convinced us that if environmental matters are ever to enter the agenda of public decisions in poor countries, they need first to enter university courses on basic economics. We felt that only in this way would the next generation of civil servants in poor countries be likely to prove less insensitive. In order to have this happen, though, university teachers of economics had to be convinced that it is not difficult to graft environmental problems into their core courses in economics. Before we could do that, however, we had to prepare teaching material, and then teach the subject to university teachers. For all this we needed money, and so a sponsor. Here lay the snag. And it lay there until 1988 when Lal Jayawardena, Director of the new World Institute for Development Economics Research (WIDER) in Helsinki, asked Mäler and me to initiate a programme of research and teaching on the general theme of the environment and emerging development issues.

The programme is now in its third year, and we have at hand 40 commissioned articles on a wide range of themes on both analytical and applied environmental economics. We are preparing an elementary textbook on the subject, as a prototype for the kind of texts which could be written by economics teachers in poor countries. Until recently, much the most illuminating applied work on the rural environment in poor countries had been done by social activists and anthropologists (e.g. Agarwal *et al.*, 1982; Howe, 1986). Applied development economists chose to display an innocence of the subject. But both in style and in notions of what constitutes social evidence, this work constitutes a different universe from the one we economists occupy. Mäler's and my plan was to codify our way of thinking on environmental problems in poor countries.[26] The elusive object we hoped to capture is the way poverty, fertility, and degradation of the local environmental resource base are connected in the drylands of sub-Saharan Africa and the Indian sub-continent. Inevitably, the process of thinking through these interconnections has influenced my own analytical work on undernourishment and the persistence of poverty in poor economies during their growth process.

15. The economics of undernourishment

While writing my book on the control of environmental resources for UNCTAD UNEP I had read a pair of articles by Ernest Feder (1977, 1979), which are even now relatively unknown. In them the author described how massive private investment in the expansion of beef-cattle production in the fragile ecological conditions of the Amazon basin has been supported by domestic governments in the form of tax concessions and the provision of infrastructure, financed in part by loans from international agencies, such as the World Bank. Over the 1960s and 1970s protein intake by the rural poor declined even while the production of beef protein increased dramatically.[27] Feder claimed that the degradation of vast tracts of environmental resources of worth to local communities had resulted in large numbers of small farmers and agricultural labourers becoming disenfranchised from the economy.

Since I did not doubt the claim, I reported it in my book. But as a theorist I could not make sense of it. Why should the fact that a person has lost his land to thieves disenfranchise him from the economy? Why is he not able to find employment as a wage labourer? If Feder was right, it must be that the distribution of productive assets is capable of affecting simultaneously the volume of employment and the nutrition status of the population. The problem was, the former did not follow directly from standard resource allocation theory.

At Stanford during 1983–4 I mentioned this to Debraj Ray, who was then a young Faculty member there. We felt explaining it by appealing to *ad hoc* imperfections of the labour and credit markets would not be the thing to do, as we would then have to base it on conditions prevailing in these markets in the Amazon basin, of which we knew nothing. In any event, if there is a link between the distribution of assets and involuntary unemployment, we felt it must be more fundamental. We were familiar with Mirrlees's and Stiglitz's formalization of Leibenstein's idea that the biological connection between nutrition and labour productivity influences the workings of the labour market (Leibenstein, 1957; Mirrlees, 1975; Stiglitz, 1976). This seemed a natural place from which to start.

Our aim was to build a general equilibrium construct which would encompass the Arrow–Debreu model of resource allocation. We wanted the general relationship between nutritional status and productivity to affect poor economies when there are large numbers of assetless people. The thought here was that even labour power should not be regarded as an asset: what an assetless person owns is potential labour power, nothing else. Conversion of potential into actual labour power can be realized if the person finds a means of making the conversion, not otherwise.

When a person maintains his nutritional balance, somewhere in the region of 70 per cent of his calorie consumption is spent on maintenance. It is a fixed cost involved in the production of living. The remainder is used for work and discretionary activities. A person with access to non-wage income is capable of effecting at least a part of this conversion without working for a wage. In this he enjoys an advantage over his assetless counterpart, and can undercut him in the labour market. A pure theory of involuntary unemployment and undernourishment, which links their incidence and extent to the distribution of assets, can be fashioned out of these factors.

This much we had discovered while I was at Stanford. Its ramifications had to wait until after I had returned to England and had moved to Cambridge at the beginning of 1985. Ray made two visits to me at Cambridge, at the end of which we had pretty much exhausted what we had originally set out to explore (Dasgupta and Ray, 1986, 1987, 1990). Our theory identified the assetless as the ones who are vulnerable to unemployment and malnutrition in a poor country. The disenfranchised are from this segment of the population. This was, of course, what had set us off in the first place. But Ray and I also showed that in such situations there are patterns of egalitarian asset redistributions which lead to greater economy-wide output. Agrarian reform in our papers was, of course, a metaphor for a wide range of asset redistribution governments in poor countries are frequently advised to engage in. These include public health facilities and primary education. What our theory was telling us was that this need not be at the expense of growth in national product.

We wanted to make contact with the Arrow–Debreu model. So we showed that the biological link between nutrition and productivity has no operational significance in a rich economy possessing a complete set of markets: the equilibrium wage rate is sufficiently high to clear the market, and nobody is disenfranchised. There is then a precise sense in which the Arrow–Debreu model is theoretically valid only for a rich economy.[28]

16. The state of poverty studies

Reaction to this work puzzled me. The work found resonance among general equilibrium theorists and macro-economists interested in resource allocation mechanisms involving efficiency wages (e.g. Weiss, 1990). But there was no response from economists working on poverty. To me this was surprising, because about the time we wrote our papers there was a resurgence of interest in the study of poverty in poor countries. One of the largest such studies was being undertaken at WIDER, and the World Bank was soon to devote its annual development report to the subject of poverty. Now Ray and I had written our articles with one pair of eyes on extending pure theory, and the other on providing a language for policy discussions on poverty alleviation in poor countries. Initially, I thought it was this split in intentions which was the reason for its failure to generate interest among economists studying poverty. I soon came to realize otherwise.

The problem was that the new research into poverty almost at once became oriented towards case-studies; it displayed little interest in analysis (see e.g. Underwood, 1983; Biswas and Pinstrup-Anderson, 1985; Reutlinger and Pellekaan, 1986; Berg, 1987; UNICEF, 1987; Bell and Reich, 1988; Ajit Dasgupta, 1988; George, 1988; Dreze and Sen, 1990; UNDP, 1990; World Bank, 1990). But it seemed to me that while narratives without the backing of theory can inform, they cannot illuminate. I began to feel that my friends and colleagues working on the subject of poverty thought description amounts to explanation. For example, I found that while this new literature recognized the phenomenon of undernourishment (which is a biomedical condition), it was doing nothing with the phenomenon when talking about policy. Were 'undernourishment' to be replaced by 'poverty', or 'low income' or 'low welfare', no injustice would be done to the narratives.[29]

I sensed another problem with this emerging literature. It seemed to come awfully close to identifying the incidence of poverty with pure distributional failures. For sharp, short incidents of acute deprivation it is obviously a reasonable stance to adopt. But for the more enduring forms which plague much of the globe, this restriction would not do for me. It seemed to me that the poorest countries are also the ones whose resource bases have been dwindling at a fast pace, whose per capita incomes and food consumptions have been falling, whose populations have been growing at furious rates, and which contain a substantial fraction of the world's undernourished and illiterates. I thought these could not be unrelated. Matters of poverty, undernourishment, illiteracy and disease appeared to me to be closely linked in turn to high fertility, degradation of the environmental resource base, a lack of infrastructure, and the organization of production and information. I could not see how the production, information and reproductive bases of rural economies could systematically be ignored in discussions of acute poverty. I felt that the bulk of the new poverty literature had turned its back on resource allocation theory and its potential developments, and was resorting to journalism. I was by now too much of an economist not to react to this. My Walras-Bowley Lecture to the Econometric Society in 1989 formalized a number of these thoughts and extended my work with Ray to incorporate nutrition status as a stock, or state, variable (see Dasgupta, 1989e).

Academic considerations can overwhelm one's life. During the early months of 1988 I could not put aside the very thoughts which were about to dominate my research. I felt alone in my concerns about the methodological weaknesses in the new literature on poverty, which was even then gathering momentum. But methodological discussions bored me. In any case, I knew debates on methodology rarely advance a subject, that rather it is the successful doing of it which convinces the bright graduate student, which is what ultimately counts. While I had the necessary analytical skills with which to offer an alternative to the new poverty literature, I was all too aware of my innocence of the large empirical literature which has been built round the theme of poverty over the years by nutritionists, geographers, anthropologists, demographers, epidemiologists and development economists. A short course of lectures I gave at the Woodrow Wilson School at Princeton in the spring of 1988 revealed my ignorance to me. But I did not know where to start, or indeed how to start. I had a feeling of urgency, but I did not know if I had the energy.

These thoughts I put to Andrew Schuller of Oxford University Press one evening at the European Economic Association Meeting in Bologna in 1988. I put them to him as a complaint, since it was he who was publishing much of the new poverty literature. He suggested I write a book on all of this, to be published, naturally, by him. This smacked of publishing polygamy, so I did not at once promise him the book. But I promised myself to write it, and to write it without the backing of a research grant so as to be free to write it at any pace I chose. It was necessary to give myself this flexibility, because there was the possibility my wife and I would soon move to Stanford University, and this would slow things down. In the event we did move, but it did not slow things down. It merely accelerated the pace at which I was used to working.

17. Political and civil liberties

In the spring of 1988, Kenneth Arrow called me to ask if my wife and I would consider moving permanently to Stanford. He said he and several of his colleagues in the social sciences and humanities were members of a search committee for the Directorship of a proposed interdisciplinary programme of research and teaching on social ethics (it was to be called the Program in Ethics in Society). Their thought was that I would hold the Directorship in conjunction with the Chair that went with it, which in my case would involve joint professorships at the Economics and Philosophy departments.

Rites of appointment in the USA are a fit subject for social anthropologists. It was one thing for John Shoven (the then Chairman of the Economics Department) to receive me at the airport. (We had been friends for many years, and he is a good deal younger than I.) It was another thing to find Arrow responsible for my being at the right place at the right time during my visit.

That Arrow appeared to regard it as a perfectly natural thing to do made it worse. I have never felt so uncomfortable in my life.

The ability that American academics have of being able to move places on a frequent basis never ceases to surprise me. But I felt my economics colleagues at Stanford had every reason to think our decision problem was an easy one. I had even then more friends there than at any other place. My wife had several friends there and liked the area. Moreover, from where I stood they had created the best economics department in the world. So what was our problem? Well, none except for a few small historical details: we had lived in England all these years, and were used to its ways; my parents lived in India and knew England a lot more than the West Coast; and most of my wife's family was based in England.

My colleagues at Stanford rescued us from our inability to reach a decision by asking me to take leave from Cambridge for two years. This way we could make up our minds when we had settled down there. My wife felt enthusiastic about this proposal. It was an exquisite gesture on Stanford's part, and in one of those childish moments I imagine we all experience from time to time, I promised myself to repay them by making a success of their new programme and giving everything I had to writing my book.

One of the courses I was expected to teach in the Philosophy Department at Stanford was at the interface of welfare economics and political philosophy. I knew the former well and the latter not so well. I thought it would be useful to learn something of the way in which philosophers try and assess individual and social well-being. My planned course of lectures could then be a bridge between the very abstract philosophical literature on utility and rights (which I knew) and the very concrete economics literature on the measurement of the quality of life (which I also knew).

Once again, I ran into something which bothered me. For the most part moral and political philosophers appeared to shy away from studying current data. They often retreated to the Greek Classics for their laboratory work. They seemed to have no confidence that the real things they analysed so effectively could be put to work on current problems.[30] Admittedly, well-being is a most elusive notion. But just as medical practitioners study ill health to learn about good health, it seemed to me one could get a grip on the notion of well-being by studying extreme patterns of ill-being in the modern world. That by this route I could bring modern political philosophy to bear on my understanding of the economics of destitution was, of course, an attractive thought as well.

The quantitative literature on living standards (e.g. the annual *World Development Report* of the World Bank), on the other hand, was wholly anchored to the design of socio-economic indicators. I could find no place where someone had tried to aggregate them with indicators of political and civil liberties. Indeed, but for the early work of Adelman and Morris (1967), which I discovered only later, I found no mention of quantitative indices of political and civil liberties in the economics literature.

Economists by training seek to locate the instrumental worth of anything which looks as though it may have worth. In my early work on individual rights I had sought to show their instrumental worth in a world with asymmetric information. Now I wanted to see if within poor countries political and civil liberties have on average been associated with creditable performance in the socio-economic sphere of life. My understanding of modern resource allocation theory had led me more and more towards a liberal democratic political philosophy. For me it was a case of economics sharpening my political dispositions, rather than the other way round. Now I wanted to see if the evidence pointed to my beliefs.

I wanted to concentrate on poor countries partly because otherwise Western and Eastern Europe would contaminate the data, and partly because of the common presumption people entertain that civil and political liberties are luxuries the poor cannot afford. 'Food before freedom' is the vulgar form of the thought that there must be a trade-off between the socio-economic, and the political and civil aspects of citizenship. In my Frank Paish Memorial Lecture to the Royal Economic Society in 1989 (Dasgupta, 1990), I showed that during the decade of the 1970s, of approximately 50 of the poorest countries, those with a better record on political and civil liberties on average achieved greater *improvements* in life expectancy at birth, the infant survival rate and national income per head.[31] Admittedly, correlation is not causation. On the other hand, modern resource allocation theory is not belied by the evidence either.

I now knew that my planned book had to be even broader in scope than the one I had sketched before Schuller. The plan originally was to write a book on the economics of destitution. Now it seemed to me I should attempt an inquiry into well-being and destitution, a sort of grand synthesis of the social world as inhabited by the deprived, in which an appeal would be made both to analytical and empirical material when developing the contours of what constitutes a good society in a poor world. I recalled my sense of wonder on reading Arrow's monograph in the summer of 1977, and knew if I did not try it now, I never would.

18. Well-being and destitution

On my way back from Athens to Stanford in early September 1989, I had stopped at Cambridge to settle my eldest daughter. She had chosen to remain there for the coming year so as not to interrupt her studies. I was staying in my room in St John's College. The night before leaving for Stanford I went for a stroll along the Backs, to get a feeling of why Cambridge is thought to be an enchanting place. The evening was balmy, the sort you often find at that time of year. Sitting on the Backs of my College I thought about this crazy plan of mine, both about where to live and about what to write. I was nearly 47 years old, and had still not settled down. My wife had wanted a sedentary life when we planned our future, and I had dragged her into the nomadic pattern which had shaped my life. It was then that it hit me that I had made a mess of it all. I did not see where I would get the time or energy to fulfil the many things now expected of me. I did not have the faintest idea how to go about writing my book, let alone how to find the energy and the enthusiasm to do so. But then it occurred to me that perhaps I had at last earned the right to make a fool of myself, that even if my project was doomed to failure, it was not ignoble. It was worth a try. Besides, I thought, if I did not try it, who would?

In the event, it was pointless to have worried about finding the necessary enthusiasm to teach myself new things. At Stanford I became obsessed to the point where pretty much all I thought of, or read, or talked about, was from the perspectives of the Program in Ethics in Society, my proposed book and the research programme on environment and development I was directing with Mäler at WIDER. Needless to say, they all appeared as parts of a jigsaw I was trying to understand about the way lives are led by the rural poor in poor countries. I began by exploring in collaboration with Mäler the analytical link between poverty, the fragility of property rights to the environmental resource base, and the organization of rural production in the drylands (Dasgupta and Mäler, 1991). I then offered an analysis of a parallel link between fertility and poverty among the rural poor in the drylands (Dasgupta, 1991c). In my book I have brought all this together in the context of a world where there are fixed personalized nutritional costs (i.e. personalized non-convexities) in the production of living. I have used these costs to explain the fact that household poverty can feed upon itself to persist over generations, and to confirm that those households which for one reason or the other are able to lift themselves out of the mire can cumulatively pull themselves away totally. The rural poor in the Indian sub-continent and in sub-Saharan Africa are a long way from making the demographic transition. For the urban middle classes, at least in India, it is not only within sight, a great many have already made the transition. The framework I have constructed explains why the fruits of economic growth are slow to trickle down to the poor unless there are conscious public moves to have it happen.

It must be a form of madness, not being able to think of anything other than one thing for long stretches of time. It also turns one to brigandry, and during the past two and a half years I have learnt how to snatch moments to chase up references in libraries as I flitted between lectures, meetings and visits in Japan, England, India, Finland, Sweden and Italy; and to then chase the references which emanated from these references, …, and so forth. But my intellectual homing ground became Stanford, which offered me the right mix of economists, nutritionists, demographers, philosophers and ecologists to tutor me and to give shape to my inquiry into well-being and destitution. This book, which is now nearly complete (Dasgupta, 1991d), could not have been written but for the intellectual milieu I have found unique to Stanford.

19. Envoi

It is 17 November 1991, and I am 49 years old today. Sitting and typing this in my home in Cambridge, where I have resumed my Chair on leave from Stanford, I realize that my wife and I are once again at a crossroads, for we have still to decide where we shall be. As I reflect upon this essay it comes home to me that my work to date has in a real sense been unanticipated at every stage. No one, least of all myself, had any great expectation from me. Moreover, I had to begin with little conception of what inquiry in the social sciences amounts to. So I had no grand cause, no plan, no agenda. Over the years, economics has as a result come to mean something intensely personal for me. It has come to mean warm friendships, a happy family life, intellectual wonder, and the development of a sense of awe of those who leave an imprint on our understanding of the world around us. It seems to me, if you have experienced these, you have sensed much that is valuable.

Notes

This final version contains very minor editorial changes to the one prepared and circulated in November 1991, which benefited greatly from the comments I received from Carol Dasgupta, Jeremy Edwards, Geoffrey Harcourt, I. G. Patel and Paul Seabright on the first version (October 1991). When I began writing this piece (late September 1991) I think I knew my father did not have much time left. It is a tragic weakness of Bengali culture (and I dare say of other cultures too) that some of the most urgent things remain unsaid, especially by sons to their fathers, and some of the most puzzling questions remain unasked. Letters are a medium of communication, as are autobiographical essays, and while writing this essay I realized I was actually writing it *to* him. I was at the same time trying hard to complete the book I talk about towards the end of this essay, which was also addressed to him. My father read the November 1991 version of this essay. He was frail, and was slow to read it, but he told me several times as I accompanied him and my mother back to their home in Santiniketan from my sister's home last month that he liked it. He said the way I do economics was not his way, but he also said he liked my way, and that I should stick to it. I now do not know why, but I did not ask what he meant by that.

My father died on 14 January 1992.

1. There was one exception to this: Frank Hahn. But he had left for Stanford and the London School of Economics (LSE) in 1966, and I did not come to know him professionally until we became colleagues at the LSE in 1971.
2. Assuming $U(C)$ to be a strictly increasing, strictly concave function of C (consumption), and assuming B to be the least upper bound of U, the necessary and sufficient condition was the convexity of $\log[B - U(C)]$.
3. I had overlooked the possibility that Kenneth Arrow at Stanford found the problem interesting. John Lane, who was to become a friend and colleague at the LSE some years later, had also begun working on the joint problem of population and savings for his PhD at Stanford. See Lane (1977).
4. Although I had been a student of Mirrlees for the three previous years, this was the first I heard that he had worked on the problem.

5. As far as I can recollect, Sen never asked me to check first if I was in agreement with the spirit of the UNIDO notes before accepting the collaborative arrangement; he took it for granted I would be sympathetic with the notes. Likewise, I never consulted him when completing and codifying those parts of the resulting book for which I was responsible. Nor, for that matter, did he consult me when writing his chapters. Kinship has its advantages.

   I should add that Marglin, Sen and I never met while preparing the book. This explains the non-uniformity of the book's style.
6. Blitzer, Dasgupta and Stiglitz (1981) elaborate this method more formally. See also Dasgupta (1982c).
7. This statement is true only when some further, technical conditions are met (see Mirrlees, 1972), but I am gliding over technical details here.
8. Unknown to us, Solow was solving this problem. See Solow (1974).
9. See Koopmans (1980), Dasgupta and Stiglitz (1981) and Dasgupta (1989a) for further discussions of the transition problem when the resource is a source of energy.
10. This has been confirmed since then in a number of studies of rural communities in poor countries. See, for example, Howe (1986) and Wade (1987). It is one of those cases where economic prediction has preceded confirmation.
11. In collaboration with Richard Gilbert, we subsequently investigated the joint phenomena of R&D competition and resource depletion in Dasgupta, Gilbert and Stiglitz (1982, 1983).
12. I have since then gone more fully into this in Dasgupta (1986a).
13. The model was developed independently by Loury (1979).
14. See Dasgupta and David (1991). I have reported aspects of our findings in Dasgupta (1987, 1988b, 1989b).
15. Phelps and Pollak (1968) had made a weaker observation on a somewhat different construct.
16. For example, I explored core allocations among the generations and found them to be an unusually large set (see Dasgupta, 1974c).
17. I did not know at that time that Solow had already interpreted Rawlsian savings as intergenerational maximin (see Solow, 1974).
18. Phelps and Riley (1978) confirmed this fact subsequently.
19. This we knew to be extreme, and it was suitably generalized by Arrow in his paper.
20. Calvo (1978) contains the most complete statement along these lines.
21. Narveson (1967) had noted this difficulty with Classical Utilitarianism earlier, and had recast Utilitarianism to take account of it. ('We are for making people happy, not for making happy people', Narveson, 1978.)
22. Authors whose formulations come closest to my own are Schell (1982), Seabright (1989) and Heyd (1991).
23. These theorems have been complemented by Simon (1987) and Simon and Zame (1990).
24. I have elaborated this programme of research further in Dasgupta (1982b, 1986b, 1989d). Among philosophers, the one whose formulation has come closest to mine is Hardin (1988).
25. I should add that my book had only one chapter on pollution, and none on environmental amenities. The specialized, technical chapters were on pollution, fisheries, ground-water, soil quality, forests and biodiversity. I still cannot fathom what these civil servants thought they were saying.
26. Dasgupta and Mäler (1991) presents the point of view which shaped the design of WIDER's programme on the environment and emerging development issues.
27. Much of the beef was destined for exports, for use by fast-food chains.

28. Hammond (1990) has extended this class of models considerably to accommodate the possibility of non-survival.
29. I have gone into this in greater detail in Dasgupta (1991b).
30. A notable exception is Ronald Dworkin, but his problem area is jurisprudence, a bit removed from what I was after.
31. The rogue indicator was adult literacy. Improvements in this went the other way. See also Dasgupta and Weale (1991), which provides a ranking of the world's poorest countries in terms of the Borda aggregator used on civil and political liberties, national income per head, life expectancy at birth, infant survival rate and the adult literacy rate.

References

Adelman, I. and Morris, C. T. (1967) *Society, Politics and Economic Development*. Baltimore: Johns Hopkins University Press.

Agarwal, A. *et al.* (1982) *The State of India's Environment: a citizen's report*. New Delhi: Centre for Science and Environment.

Ahmad, E. and Stern, N. (1990) *The Theory and Practice of Tax Reform for Developing Countries*. Cambridge: Cambridge University Press.

Arrow, K. J. (1973) Rawls' principle of just savings. *Swedish Journal of Economics*, **75**.

Arrow, K. J. (1974) *The Limits of Organization*. New York: Norton.

Barro, R. and Becker, G. (1989) Fertility choice in a model of economic growth. *Econometrica*, **57**.

Bell, D. E. and Reich, M. R. (eds) (1988) *Health, Nutrition and Economic Crises*. Dover, Mass.: Auburn House.

Berg, A. (1987) *Malnutrition: what can be done?* Baltimore: Johns Hopkins University Press.

Biswas, M. and Pinstrup-Anderson, P. (eds) (1985) *Nutrition and Development*. Oxford: Oxford University Press.

Blitzer, C., Dasgupta, P. and Stiglitz, J. (1981) Project appraisal and foreign exchange constraints. *Economic Journal*, **91**.

Boadway, R. (1978) A note on the treatment of foreign exchange in project evaluation. *Economica*, **45**.

Calvo, G. A. (1978) Some notes on time inconsistency and Rawls' mafiimin criterion. *Review of Economic Studies*, **45**.

Dasgupta, A. (1988) *Growth, Development and Welfare*. Oxford: Basil Blackwell.

Dasgupta, P. (1969a) Optimal growth when capital is non-transferable. *Review of Economic Studies*, **36**.

Dasgupta, P. (1969b) On the optimum rate of accumulation in a labour surplus economy. *Indian Economic Journal*, **16**.

Dasgupta, P. (1969c) On the concept of optimum population. *Review of Economic Studies*, **36**.

Dasgupta, P. (1970) An analysis of two approaches to project evaluation in developing countries. *Industrialization and Productivity* (UNIDO), Bulletin No. 15.

Dasgupta, P. (1972) A comparative analysis of the UNIDO guidelines and the OECD manual. *Bulletin of the Oxford University Institute of Economics and Statistics*, **34**.

Dasgupta, P. (1974a) Some recent theoretical explorations in the economics of exhaustible resources. In H. Gottinger (ed), *Systems Approaches and Environmental Problems*, Gottingen: Vandenhoeck and Ruprecht.

Dasgupta, P. (1974b) On some problems arising from Professor Rawls' conception of distributive justice. *Theory and Decision*, **4**.

Dasgupta, P. (1974c) On some alternative criteria for justice between generations. *Journal of Public Economics*, **3**.

Dasgupta, P. (1974d) On optimum population size. In A. Mitra (ed), *Economic Theory and Planning: essays in honour of A. K. Dasgupta*. Delhi: Oxford University Press.

Dasgupta, P. (1980) Decentralization and rights. *Economica*, **47**.

Dasgupta, P. (1982a) Resource depletion, research and development and the social rate of discount. In R. Lind (ed), *Discounting for Time and Risk in Energy Policy*. Baltimore: Johns Hopkins University Press.

Dasgupta, P. (1982b) Utilitarianism, information and rights. In A. Sen and B. Williams (eds), *Utilitarianism and Beyond*. Cambridge: Cambridge University Press.

Dasgupta, P. (1982c) *The Control of Resources*. Oxford: Basil Blackwell; Cambridge, Mass.: Harvard University Press, and Delhi: Oxford University Press.

Dasgupta, P. (1986a) The theory of technological competition. In J. E. Stiglitz and F. Mathewson (eds), *New Developments in the Analysis of Market Structure*. London: Macmillan.

Dasgupta, P. (1986b) Positive freedom, markets and the Welfare State. *Oxford Review of Economic Policy*, 2. Reprinted in D. Helm (ed), 1989: *The Economic Borders of the State*. Oxford: Oxford University Press.

Dasgupta, P. (1987) The economic theory of technology policy. In P. Dasgupta and P. Stoneman (eds), *Economic Policy and Technological Performance*. Cambridge: Cambridge University Press.

Dasgupta, P. (1988a) Lives and well-being. *Social Choice and Welfare*, **5**.

Dasgupta, P. (1988b) Patents, priority and imitation or, The economics of races and waiting games. *Economic Journal*, **98**.

Dasgupta, P. (1989a) Exhaustible resources. In L. Friday and R. Laskey (eds), *The Fragile Environment*. Cambridge: Cambridge University Press.

Dasgupta, P. (1989b) Economic organization and technological change. In A. Silberston (ed), *Technology and Economic Progress*. London: Macmillan.

Dasgupta, P. (1989c) Population size and the quality of life. *Proceedings of the Aristotelian Society*, supplementary volume LXIII.

Dasgupta, P. (1989d) Power and control in the good polity. In A. Hamlin and P. Pettit (eds), *The Good Polity*. Oxford: Basil Blackwell.

Dasgupta, P. (1989e) The allocation of hunger. Walras-Bowley Lecture, Econometric Society. *Econometrica*, forthcoming.

Dasgupta, P. (1990) Well-being and the extent of its realization in poor countries. *Economic Journal*, supplement, **100**.

Dasgupta, P. (1991a) Savings and population: normative considerations. Mimeo, Stanford University.

Dasgupta, P. (1991b) Nutrition, non-convexities, and redistributive policies. *Economic Journal*, **101**.

Dasgupta, P. (1991c) Population, resources and poverty. *Ambio* (1992), **21**, forthcoming.

Dasgupta, P. (1991d) *An Inquiry into Well-Being and Destitution*. Oxford: Oxford University Press.

Dasgupta, P. and David, P. (1987) The economics of science and technology. In G. Feiwel (ed), *Arrow and the Ascent of Modern Economic Theory*. London: Macmillan.

Dasgupta, P. and David, P. (1991) Priority, secrecy, patents and the institutions of science and technology. *Science in Context*, forthcoming.

Dasgupta, P., Gilbert, R. and Stiglitz, J. (1982) Invention and innovation under alternative market structures: the case of natural resources. *Review of Economic Studies*, **49**.

Dasgupta, P., Gilbert, R. and Stiglitz, J. (1983) Strategic considerations in invention and innovation: the case of natural resources. *Econometrica*, **51**.

Dasgupta, P., Hammond, P. and Maskin, E. (1979) The implementation of social choice rules: some general results in incentive compatibility. *Review of Economic Studies*, **46**.

Dasgupta, P. and Heal, G. M. (1974) The optimal depletion of exhaustible resources. *Review of Economic Studies*, **41** (special number).

Dasgupta, P. and Heal, G. M. (1979) *Economic Theory and Exhaustible Resources*. Cambridge: Cambridge University Press.

Dasgupta, P., Heal, G. M. and Majumdar, M. (1977) Resource depletion and research and development. In M. Intriligator (ed), *Frontiers of Quantitative Economics*, Vol. IIIB. Amsterdam: North-Holland.

Dasgupta, P. and Mäler, K.-G. (1991) The environment and emerging development issues. *Proceedings of the Annual World Bank Conference on Development Economics*, 1990.

Dasgupta, P., Marglin, S. and Sen, A. (1972) *Guidelines for Project Evaluation*. New York: United Nations.

Dasgupta, P. and Maskin, E. (1986a) The existence of equilibrium in discontinuous economic games. 1: Theory. *Review of Economic Studies*, **53**.

Dasgupta, P. and Maskin, E. (1986b) The existence of equilibrium in discontinuous economic games. 2: Applications. *Review of Economic Studies*, **53**.

Dasgupta, P. and Maskin, E. (1987) The simple economics of research portfolios. *Economic Journal*, **97**.

Dasgupta, P. and Ray, D. (1986) Inequality as a determinant of malnutrition and unemployment: Theory. *Economic Journal*, **96**.

Dasgupta, P. and Ray, D. (1987) Inequality as a determinant of malnutrition and unemployment: Policy. *Economic Journal*, **97**.

Dasgupta, P. and Ray, D. (1990) Adapting to undernourishment: the biological evidence and its implications. In J. Dreze and A. Sen (eds), *The Political Economy of Hunger: Entitlement and Well-Being*. Oxford: Clarendon Press.

Dasgupta, P. and Stiglitz, J. E. (1972) On optimal taxation and public production. *Review of Economic Studies*, **39**.

Dasgupta, P. and Stiglitz, J. E. (1974) Benefit cost analysis and trade policies. *Journal of Political Economy*, **83**.

Dasgupta, P. and Stiglitz, J. E. (1980a) Industrial structure and the nature of innovative activity. *Economic Journal*, **90**.

Dasgupta, P. and Stiglitz, J. E. (1980b) Uncertainty, industrial structure and the speed of R&D. *Bell Journal of Economics*, **11**.

Dasgupta, P. and Stiglitz, J. E. (1981) Resource depletion under technological uncertainty. *Econometrica*, **49**.

Dasgupta, P. and Stiglitz, J. E. (1982) Market structure and resource depletion: a contribution to the theory of intertemporal monopolistic competition. *Journal of Economic Theory*, **28**.

Dasgupta, P. and Stiglitz, J. E. (1988) Learning by doing, market structure and industrial and trade policies. *Oxford Economic Papers*, **40**.

Dasgupta, P. and Weale, M. (1992) On measuring the quality of life. *World Development*, 1992.

David, P. A. (1988) The economics of patronage and the historical roots of the institutions of open science. HTIP Working Paper, Center for Economic Policy Research, Stanford University.

Diamond, P. A. and Mirrlees, J. A. (1971) Optimal taxation and public production, I & II. *American Economic Review*, **61**.

Diamond, P. A. and Mirrlees, J. A. (1976) Private constant returns and public shadow prices. *Review of Economic Studies*, **43**.

Dinwiddy, C. and Teal, F. (1987) Shadow prices for non-traded goods in a tax-distorted economy: formulae and values. *Journal of Public Economics*, **33**.

Dreze, J. and Sen, A. (1990) *Hunger and Public Action*. Oxford: Oxford University Press.

Ehrlich, P., Ehrlich, A. and Holdren, J. (1977) *Ecoscience: population, resources and the environment*. San Francisco: W. H. Freeman.

Feder, E. (1977) Agribusiness and the elimination of Latin America's proletariat. *World Development*, **5**.

Feder, E. (1979) Agricultural resources in underdeveloped countries: competition between man and animal. *Economic and Political Weekly*, **14**.

Findlay, R. and Wellicz, S. (1976) Project evaluation, shadow prices, and trade policy. *Journal of Political Economy*, **84**.

Forrester, J. (1971) *World Dynamics*. Cambridge, Mass.: Wright-Allen Press.

George, V. (1988) *Wealth, Poverty and Starvation: an international perspective*. Hemel Hempstead, Herts.: Wheatsheaf.

Gordon, H. S. (1954) Economic theory of common property resources. *Journal of Political Economy*, **63**.

Hammond, P. (1990) Irreducibility, resource relatedness and survival with individual non-convexities. EUI Working Paper No. 89/427. Florence: European University Institute.

Hardin, R. (1988) *Morality within the Limits of Reason*. Chicago: Chicago University Press.

Heal, G. M. (1975) Economic aspects of natural depletion. In D. W. Pearce and J. Rose (eds), *The Economics of Depletion*. London: Macmillan.

Heyd, D. (1991) *Genethics: the morality of procreation*. Los Angeles: University of California Press.

Hotelling, H. (1931) The economics of exhaustible resources. *Journal of Political Economy*, **40**.

Howe, J. (1986) *The Kuna Gathering*. Austin, Texas: University of Texas Press.

Koopmans, T. C. (1980) The transition from exhaustible to renewable or inexhaustible resources. In C. J. Bliss and M. Boserup (eds), *Economic Growth and Resources*, Vol. 3*: Natural Resources*. London: Macmillan.

Lane, J. (1977) *On Optimum Population Paths*. Berlin: Springer-Verlag.

Leibenstein, H. (1957) *Economic Backwardness and Economic Growth*. New York: Wiley.

Little, I. M. D. and Mirrlees, J. A. (1968) *Manual of Industrial Project Analysis for Developing Countries*, Vol. II*: Social Cost–Benefit Analysis*. Paris: OECD.

Loury, G. (1979) Market structure and innovation. *Quarterly Journal of Economics*, **93**.

Mäler, K.-G. (1974) *Environmental Economics: a theoretical enquiry*. Baltimore: Johns Hopkins University Press.

Mirrlees, J. A. (1972) On producer taxation. *Review of Economic Studies*, **39**.

Mirrlees, J. A. (1975) A pure theory of underdeveloped economies. In L. Reynolds (ed), *Agriculture in Development Theory*. New Haven: Yale University Press.

Narveson, J. (1967) Utilitarianism and new generations. *Mind*, **76**.

Narveson, J. (1978) Future people and us. In R. I. Sikora and B. Barry (eds), *Obligations to Future Generations*. Philadelphia: Temple University Press.

Nordhaus, W. (1973) The allocation of energy resources. *Brookings Papers on Economics Activity*, **3**.

Parfit, D. (1982) Future generations: further problems. *Philosophy and Public Affairs*, **11**.

Parfit, D. (1984) *Reasons and Persons*. Oxford: Oxford University Press.

Phelps, E. and Pollak, R. (1968) Second-best national savings and game-equilibrium growth. *Review of Economic Studies*, **35**.

Phelps, E. S. and Riley, J. G. (1978) Rawlsian growth: dynamic programming of capital and wealth for intergenerational 'Maximin' justice. *Review of Economic Studies*, **45**.

Rawls, J. (1972) *A Theory of Justice*. Oxford: Clarendon Press.

Reutlinger, S. and Pellekaan, H. (1986) *Poverty and Hunger: issues and options for food* *security in developing countries*. Washington, DC: World Bank Publications.

Rothschild, M. and Stiglitz, J. E. (1976) Equilibrium in competitive insurance markets: an essay on the economics of imperfect information. *Quarterly Journal of Economics*, **90**.

Schell, J. (1982) *The Fate of the Earth*. New York: Avon.

Scherer, F. M. (1970) *Industrial Market Structure and Economic Performance*. Boston: Houghton Mifflin.

Schumpeter, J. (1934) *The Theory of Economic Development*. Cambridge, Mass.: Harvard University Press.

Seabright, P. (1989) Creating persons. *Proceedings of the Aristotelian Society* (supplementary volume LXIII).

Simon, L. K. (1987) Games with discontinuous payoffs. *Review of Economic Studies*, **54**.

Simon, L. K. and Zame, W. R. (1990) Discontinuous games and endogenous sharing rules. *Econometrica*, **58**.

Solow, R. M. (1974) Intergenerational equity and exhaustible resources. *Review of Economic Studies*, **41** (special number).

Spence, A. M. (1974) *Market Signalling: informational transfer in hiring and related screening processes*. Cambridge, Mass.: Harvard University Press.

Srinivasan, T. N. and Bhagwati, J. N. (1978) Shadow prices for project selection in the presence of distortions: effective rates of protection and domestic resource costs. *Journal of Political Economy*, **86**.

Stiglitz, J. E. (1976) The efficiency-wage hypothesis, surplus labour and the distribution of income. *Oxford Economic Press*, **28**.

Stiglitz, J. E. and Dasgupta, P. (1971) Differential taxation, public goods and economic efficiency. *Review of Economic Studies*, Symposium on Public Finance, **38**.

Stiglitz, J. E. and Dasgupta, P. (1982) Market structure and resource depletion: a contribution to the theory of intertemporal monopolistic competition. *Journal of Economic Theory*, **28**.

Underwood, B. (ed) (1983) *Nutrition Intervention Strategies in National Development*. New York: Academic Press.

UNDP (1990) *Human Development Report*. New York: United Nations Development Programme.

UNICEF (1987) *The State of the World's Children*. New York: Oxford University Press.

Wade, R. (1987) The management of common property resources: finding a cooperative solution. *World Bank Research Observer*, **2**.

Weiss, A. (1990) *Efficiency Wages: models of unemployment, layoffs and wage dispersion*. Princeton, NJ: Princeton University Press.

World Bank (1990) *World Development Report*. New York: Oxford University Press.

2

Takashi Negishi

General equilibrium theory and the history of economics

1. An encounter with economics and after

Perhaps my case is peculiar in comparison with other contributors to this volume, since I started my economics with the study of the classical school of economics, and am still interested in the history of economics. My first teacher of economics, Shigeru Aihara, ordered students in the freshman course in economics to study Adam Smith, Ricardo and Marx, i.e. to read *Wealth of Nations*, *Principles* and *Das Kapital*, rather than to read modern textbooks. This was not very unusual in Japan almost 40 years ago. Japan was then a less developed country as far as economics was concerned. Japanese economists were trying to import theories developed in Western countries rather than to develop their own theories based on Japanese economy and society. This is because they wished to analyse and criticize the backwardness and peculiarity of Japanese society and economy by applying theories developed in the advanced and model societies of Western countries. Being a Marxian, Aihara might not be interested in economics after the marginal revolution. Or, perhaps, he might consider that we must study classical economics developed in the formation period of modern economy in Western countries to apply it on and to criticize Japanese society and economy, which were yet to be modernized. Anyway, this is the reason why I had to start my study of economics with the study of the classical school.[1]

The study of classical economics was not uninteresting but very difficult, since the literal arguments of classical economists seemed to me ambiguous and their propositions shown by numerical examples did not convince me of their general applicability. I wondered why mathematics could not be used in economics, as in astronomy and meteorology, which I was interested in but thought too difficult for me to major in. I was, naturally, attracted by the early mathematical economics of Cournot. I was very lucky to have Yasuhiko Oishi as my undergraduate adviser, who was one of the few non-Marxian economists in the University of Tokyo at that time and suggested I read Hicks's *Value and Capital* and Keynes's *General Theory*. I gave up studying classical economics and was first interested in the mathematical models of Keynesian macro-dynamics which was a very popular subject in the early 1950s. Being aware of the existence of an active group of Japanese mathematical economists, such as Yasui, Morishima, Ichimura, Nikaido, Inada and Uzawa, I decided to do graduate study on economic theory, wishing to join the group if possible.

My research on the general equilibrium theory was started in my first two years (1956–8) of graduate study at Tokyo, and then was refined in K. J. Arrow's project at Stanford, which was supported by the Office of Naval Research. I returned to Tokyo in 1960, finished my graduate study in 1965 and began to teach. Making the acquaintance of M. C. Kemp at that time, I was also interested in the application of the general equilibrium theory to the problems of international trade. My studies carried out in the late 1950s and 1960s are collected in my *General Equilibrium Theory and International Trade* (1972). One of the problems I tackled in the 1970s was the micro-foundations of macroeconomics, although even in my undergraduate days I had felt that something should be done to connect the world of Hicks's *Value and Capital* and that of Keynes's *General Theory*. Results of my research are collected in *Microeconomic Foundations of Keynesian Macroeconomics* (1979).[2]

This study of Keynesian economics from the point of view of general equilibrium theory led me to the clear recognition of the existence of different points of view in current economics, each of which has its own origin and predecessors. My interest in the history of economics was revived. Being elected as a member of the Science Council of Japan in 1985 and engaged in editing an official report on scientific researches in Japan, I found that, except for mathematical economics and econometrics, most of the Japanese contributions to economics remained unknown internationally, though they were regarded very highly by Japanese economists themselves. The history of economics is no exception. This fact also encouraged my study of the history of economics. What I could study was the history of economics from the point of view of modern theory, which implies following the works of Samuelson and Morishima. My studies in the 1980s are collected in *Economic Theories in a Non-Walrasian Tradition* (1985) and *History of Economic Theory* (1989a).[3]

2. General equilibrium theory

General equilibrium theory was initiated by the French economist Léon Walras in the 1870s at Lausanne (Switzerland). It may be called a paradigm of modern economics, in the sense of Kuhn. Most economists consider that the normal science of economics consists in refinements and development of the Walrasian general equilibrium theory. In short, the gist of Walrasian theory is to grasp the economy by the equilibria of an infinite number of consumers' households and firms, and the equilibria of demand and supply in markets of multiple commodities.

According to an internationally well-known advanced textbook of general equilibrium theory, *General Competitive Analysis* (1971) by K. J. Arrow and F. H. Hahn, my contributions to the general equilibrium theory are recognized in the following problems:

1. An alternative proof of the existence of a general equilibrium by the use of the optimum property of competitive equilibria.
2. The introduction of imperfect competition into the general equilibrium theory.
3. Stability analysis of equilibrium, particularly the study of the so-called non-*tâtonnement* processes.

Let us explain them briefly (see Arrow and Hahn, 1971, p. 127 for (1), pp. 167–8 for (2) and pp. 322 and 346 for (3)).

Existence of equilibrium

Perfect competition is assumed in the Walrasian theory so that consumers and firms are assumed to take prices in markets as given. Their demand and supply of commodities can, therefore, be obtained as functions of prices of commodities, and a general equilibrium in which demand and supply are equalized for all commodities is given as a solution for unknown prices of simultaneous equations. The problem of the existence of a general equilibrium is whether there exists an economically meaningful solution of these equations. Walras could not solve the problem, since he merely demonstrated that the number of independent equations is equal to the number of unknowns. The proof for this problem was given in the 1950s by Arrow and Debreu, and others.

My own method to prove the existence of the general equilibrium is, unlike those of others, not to consider it as a solution of equations of the equality of demand and supply, but to note that social welfare is maximized in a certain sense at the general equilibrium under perfect competition and to consider it as a solution of the maximization of a social welfare function $\Sigma_i a_i u_i$, where u_i is the utility of the ith consumer and a_i is a positive constant equalized to the inverse of the marginal utility of income of the ith consumer so that his budget constraint can be satisfied at equilibrium. The maximization is done, of course, being subject to the availability (including production) of commodities, and Lagrange's multipliers corresponding to availability conditions of commodities can be interpreted as equilibrium prices which show the scarcity of commodities. In other words, the existence of a competitive general equilibrium is proved by searching a Pareto-optimal allocation of resources (i.e. a proper set of a_i), at which budget constraints of all the consumers are satisfied with efficiency prices.

This proof, which was first published in *Metroeconomica* (1960), is merely an alternative proof of the existence problem which was already solved by others. It has, however, its own merit since it makes clear some mutual implications of the existence and optimality problems of a competitive equilibrium. Furthermore, for the numerical computation of a general equilibrium, it is convenient to use the method of this proof, since it considers the general equilibrium as a solution of a kind of maximization problem (see Negishi, 1960, 1972, pp. 11–27; Hahn, 1990; Diewert, 1970; Ginsburgh and Waelbroeck, 1976).

Imperfect competition

We must admit that the assumption of perfect competition is not realistic, particularly since it implies that firms have no power to make changes in prices. Although it is possible to emphasize that perfect competition is efficient and idealistic from the point of view of normative economics, it is necessary, from the point of view of positive economics, to drop the assumption of perfect competition and to introduce the assumption of imperfect competition into the general equilibrium model of an economy, so that firms can make, rather than have to take, prices.

Price-making behaviour of a firm can be described by the assumption that the firm faces a downwardly sloping demand curve, so that the price of a commodity can be raised by the firm through the reduction of its supply. The question is how to assume the nature of such a demand curve, i.e. the relation between the price and the quantity. It is too much to assume that a firm has such perfect information that any point on the demand curve can be actually realizable, so that the firm can tell the exact quantity of the commodity it can sell at any price it charges to its customers. A more reasonable assumption is to consider that a firm perceives a subjective demand curve from the limited data of the price it charged and the quantity it could sell at such a price.

By assuming that firms constantly revise their perceived demand curves on the basis of the newest data of the realized combination of the price and quantity, I introduced imperfect competition into the general equilibrium theory in my article published in the *Review of Economic Studies* (1960–1). Although it was done under very stringent assumptions, it was perhaps the first attempt to introduce imperfect competition into a mathematical model of Walrasian general equilibrium theory, which was followed and developed by many others in the 1970s and 1980s. As will be explained below, furthermore, the idea of the perceived demand curve is very useful to consider not only imperfect competitions, but also Keynesian problems in general equilibrium analysis (see Negishi, 1960–1, 1972, pp. 103–15; Hart, 1984; Hahn, 1977).

Stability of equilibrium

It has been considered in general equilibrium theory that the adjustment process in which the price of a commodity is changed by the difference between its demand and supply will eventually reach a general equilibrium in which demand and supply are equalized for all commodities. This is the so-called stability problem of the Walrasian *tâtonnement* process. Though Walras's own consideration of this problem was insufficient, the stability of *tâtonnement* was demonstrated in the late 1950s by several authors including myself under the assumption of the gross substitutability that an increase in the price of a commodity increases the difference between demand and supply of all the other commodities.[4]

In Walrasian *tâtonnement*, however, a very stringent assumption is implicitly made so that no exchange of commodities is actually made until all the markets are cleared and all prices cease to be changed. In other words, recontract is always possible in disequilibria and commodities are exchanged only in general equilibrium. One might perhaps use this *tâtonnement* assumption to study highly organized markets such as commodity exchanges. It is, however, too much to assume this for all the markets in an economy. It is necessary to consider the stability of a general equilibrium through the so-called non-*tâtonnement* processes in which contracts to exchange commodities are actually carried out at disequilibria in which prices are changed by differences between demand and supply.

I confirmed first that a non-*tâtonnement* process is as stable as *tâtonnement* in the sense that it is also stable under the assumption of gross substitutability. Then, F. H. Hahn and I demonstrated, in *Econometrica* (1962), that it is stable even without such an assumption, if exchange transactions at disequilibria follow the so-called short-side principle so that demand or supply, whichever is smaller, can be realized as planned. Although these results are obtained only in the case of pure exchange models without production, it turns out that the role of money is very important in this non-*tâtonnement* process while it is very limited in *tâtonnement*. As will be explained below, furthermore, the study of non-*tâtonnement* processes provides many insights into the study of Keynesian models in which quantities, not prices, are adjusted in the face of demand supply disequilibria.[5]

These studies of the theory of general equilibrium were followed by their applications, particularly the applications to the international economic problems. Perhaps my contributions to be mentioned are the problems of the infant industry protection and the customs union.[6]

The infant industry

The so-called infant industry dogma (arguments for a tariff aiming at the protection of an infant industry) has a long history and has generally been accepted by international trade theorists as the only serious exception to the arguments for free trade. It is defined by J. S. Mill as the protection of a domestic new industry for the finite period of learning by experience in order to catch up with foreign mature industries. Bastable argued that the discounted present value of the social gains obtainable from the industry in the future should be larger than the social cost of protection. Kemp criticized, however, the Mill–Bastable dogma which insists that the industry should be protected if it passes these two conditions, and argued that protection is not required, even if these two conditions are satisfied, unless there exists no dynamic technological externality so that only the firm that actually carries on production gains in experience and the accumulated knowledge becomes the exclusive property of that firm. The reason insisted upon was that the private incentive to undertake investment in the learning period is sufficient, since losses of the unprotected firm in its learning period are more than offset by the later profit (Bastable condition).

In *Economic Record* (1968), I challenged the above argument and argued that the protection of the infant industry is necessary even if there is no technological externality, since there are dynamic internal economies due to indivisibilities. The demonstration is given in terms of general equilibrium analysis of the dynamically optimal resource allocation in welfare economics as well as in terms of a heuristic partial equilibrium analysis of consumers' surplus. Investment necessary in the learning period is not marginal but lumpy, since the cost differential is not likely to be infinitesimal between domestic infant and foreign mature industries. In view of the increase in consumers' surplus caused by the reduction of the price, then, such an investment may be socially desirable, even if the profit of the industry is not sufficient to replace the cost of the investment. If the country is not small, changes in foreign consumers' surplus should also be taken into consideration, since it is possible that the protected industry will become an export industry, as was the case of the Japanese automobile industry. Subsequently, Corden (1974) extended my discussion by using the concept of pecuniary external economies (Negishi, 1968, 1972, pp. 90–9; Corden, 1974, pp. 256–7).

Customs union

A customs union can at most be second best, since it is merely a partial liberation of trade within an area while world-wide free trade is the condition for the optimal resource allocation from the world's point of view. The basic model in the economics of the customs union is that of two commodities and three countries the home country, the partner country and the foreign country (non-member country or the rest of the world). Pioneering attempts to study customs unions were made in terms of partial equilibrium analysis of the home country, being based on the assumptions of infinite elasticity of the foreign and partner supply and the non-existence of tariffs imposed by foreign and partner countries. Under such assumptions, however, the problem itself would disappear since there is no incentive for the home country to impose any tariff on its imports from the foreign country. Vanek initiated the general equilibrium analysis of the customs union, which is free from such assumptions, by using geometry extensively.

I followed him and proved some of his conjecture rigorously in terms of analysis. Despite his opinion to the contrary, it was shown that the analytical method is more useful for some problems of the customs union. Firstly, it is shown that internal free trade is Pareto-optimal for the members of the union, since member countries have no incentive to impose tariffs on their imports from the rest of the world, which is higher than the so-called optimal rate. Secondly, the customs union with internal free trade is shown not to be Pareto-optimal from the point of view of the world at large, since member countries should impose import tariffs on one another for world optimality. My approach is to emphasize the optimizing behaviour of member countries when considering the optimality of the union, rather than to ask whether a country should join the union when the rate of tariff on imports from the rest of the world is given arbitrarily. This approach was followed and extended by Kemp (1969b, pp. 8, 125–39, 148; see also Negishi, 1969, 1972, pp. 179–88).

Micro-economic foundations of Keynesian equilibrium

Though I mentioned that Walrasian general equilibrium theory is a paradigm of modern economics, Keynesian economics may perhaps be called another paradigm of modern economics. While the former theory is a micro-economics which starts with the analysis of the behaviour of individual consumers and firms, the latter was developed as a macro-economic theory which is composed mainly of such aggregate concepts as gross national products, national incomes, etc. There is no problem if two theories differ only in this sense. There is, however, an essential inconsistency between two theories, since in Walrasian economics an equilibrium is defined as the equality of demand and supply and unemployment is always regarded as a temporal disequilibrium, while in Keynesian economics an equilibrium with unemployment is considered more general than the full employment equilibrium.

Although the appearance of Keynes's *General Theory* (1936) was called the Keynesian revolution, it was not a thoroughgoing revolution, since it could not penetrate micro-economics. Subsequently a Keynesian counter-revolution took place, being based on such new developments in Walrasian economics as theories of expectations, uncertainty, adjustment cost and disequilibrium, which can explain unemployment in non-Keynesian ways. The partial success of the Keynesian counter-revolution is, of course, due to the fact that Keynesian economics is a macro-economics and does not have its own micro-economics.

Apart from small, short-lived recessions, however, we still need Keynesian economics to explain persistent large-scale depression and unemployment. This is why my interest turned in the 1970s towards the general equilibrium theory of unemployment or micro-economic foundations of Keynesian macro-economics.

In Walrasian general equilibrium theory, competitive suppliers can supply whatever amount they like at the market price they take as given, since demand and supply are always equalized by a quick adjustment of the price. In a non-Walrasian general (dis)equilibrium theory, however, individual suppliers cannot supply the amount they like at the given market price and have to face quantity constraints, since the price is sticky or rigid and does not change so as to equate demand and supply. In such a fixed price model, therefore, quantities traded, rather than prices, have to be adjusted so as to clear the markets. The problem is similar to that of non-*tâtonnement*, so far as it is concerned with how quantities traded are determined in disequilibrium of demand and supply. Usually, the quantity is determined by the short-side principle which Hahn and I used in our study of non-*tâtonnement* processes.[7]

I had no objection to the quantity constraint or adjustment theory so far as to consider that the crux of Keynesian theory is the adjustment of quantities in demand and supply disequilibrium. I was dissatisfied, however, with such a model in which price is simply given exogenously, since we have to explain why price is sticky in the demand and supply disequilibrium rather than assume that it does not change in spite of disequilibrium. When the market is not cleared at the current price and it is impossible to trade the amount they like, competitive demanders or suppliers have two choices, either to admit price changes unfavourable to them, or to accept current constraints on quantities. The so-called fixed price quantity constraint theory concentrates on the second choice only and pays no attention to the first one. Of course, we assume perfect competition in which suppliers take, rather than make, prices. But the price-taking behaviour of suppliers implies that they are unable to make price changes favourable to them, i.e. to raise the price above the market one. Even the competitive suppliers can, therefore, reduce the price if they wish to. We have to explain, then, why they do not.

The typical situation the Keynesian theory of unemployment equilibrium deals with is the one where supply exceeds demand and supplying firms cannot sell the amount they like. We have to explain why prices are not reduced and only quantities traded are adjusted to quantities demanded. For this purpose, I introduced perceived demand curves which I considered above in the general equilibrium theory of imperfect competition. Unlike the case of the Walrasian market where information is perfect, an increase in demand caused by a firm's reduction of price cannot generally be expected to be very large, since it is not generally known to customers of other firms which keep the price unchanged. If a firm raises the price, however, it has to expect a large reduction in demand, since its current customers leave it to search for other firms which keep the price unchanged. Then, a competitive firm which cannot raise the price above the given market price perceives its demand curve kinked at the point of the current price and demand quantity constraint. The firm does not reduce the price of its product and adjust its output to the current demand constraint, if the marginal revenue from increasing its output, which is likely to be very small, is smaller than the marginal cost while that from decreasing its output, which is equal to the price, is larger than the marginal cost (Negishi, 1974, 1979, pp. 87–98, 247–57; Itoh and Negishi, 1987, pp. 89–93; Cuddington, Johansson and Löfgren, 1984, pp. 40–4; Malinvaud, 1985, p. ix; Reid, 1981, pp. 65–6).

As for the labour market, one of the most interesting explanations of unchanged wages is Azariadis's theory of implicit contracts between firms and labourers. The difference between firms and labourers with respect to risk aversion can explain why transactions in the labour market typically involve implicit long-term contracts with unchanged wage rates. Labourers are averse to risk and prefer to be employed at lower average wages with a small variance through time than at higher average wages with a larger variance. Firms are risk neutral and are willing to accommodate labourers in their interest of obtaining cheaper labour. I showed, furthermore, that implicit contracts can be with a variable level of employment, if firms are faced with demand quantity constraints and keep their product prices unchanged. In other words, full employment is not assured and unchanged rates of wage and unemployment can coexist (Negishi, 1979, pp. 227–35).

3. History of economic theory

Economics is a science which cannot dispense with its history. The newest version of macro-economics, for example, had to characterize itself as new *classical*, while there are many modern 'neo-' or 'post-' schools in the current theory, such as neo-Austrian, neo-classical, neo-Ricardian, neo-institutionalism, neo-Walrasian, post-Keynesian, etc., the implications of which cannot be fully understood without proper knowledge of the history of economics. This situation can perhaps be explained by Lakatos's theory of scientific research programmes (SRPs), that a school in the current economic theory is merely the newest version of each SRP which keeps its hard core unchanged through many versions.[8] With an ever-growing emphasis on formal techniques in mainstream economics, however, many economic theorists are now no more likely to develop their interest in the history of their science than are ordinary natural scientists. This is very unfortunate. To develop our science in the right direction, I do believe that more theoretical resources should be put into the study of the history of economics.[9] Though my own research is fairly new, some of it is already being recognized by historians of economics and those who are interested in the history of our science, as will be seen below.[10]

Adam Smith (1)

The division of labour that improves labour productivity is the key characteristic and driving force in Adam Smith's *Wealth of Nations*. He gave two propositions concerning the division of labour: that the division of labour is limited by the extent of the market, and that the accumulation of capital must be previous to the division of labour, so labour can be more and more subdivided in proportion only as capital is previously more and more accumulated. While the first proposition plays an important role in Book I of *Wealth of Nations*, which is Smith's theory of prices and distribution, the second proposition is first introduced and emphasized in the introduction of Book II which is his theory of capital and development. It is, then, very strange that Smith tried to explain his famous doctrine of the hierarchy or the natural order of investment, headed by agriculture, followed firstly by manufacturing, then by inland trade and finally by foreign trade, in this Book II only in terms of the creation of employment or the capital–labour ratio, entirely without recourse to the second proposition. Since the doctrine is very important as the theoretical foundation to his argument in Book III (economic history) and Book IV (economic policy), and Smith's own exposition has been regarded as admittedly confused, it is necessary to reconstruct Smith's doctrine on the foundation of the guiding principle of Book II that capital accumulation should be previous to the division of labour.

My reconstruction is as follows. The natural order of investment is concerned with exactly how capital accumulation leads to improvement in productivity due to the division of labour; clearly the investment must start in an unspecialized, self-sufficing single industry and then gradually proceed so that industries are more and more subdivided and specialized. Agriculture can be considered as such an inclusive and unspecialized single industry if we include household and coarser manufactures in agriculture. When enough capital is accumulated to support a manufacture as an independent and specialized industry other than agriculture, investment should be and actually is made so as to develop the occasional jobs in the neighbourhood of artificers into a regular manufacture for more distant sale. As capital accumulates, the division of labour advances to inter-district specialization of local manufactures. Investment in home trade should be made only when the accumulation of capital has already reached the stage where inter-district specialization is possible. The highest stage of the division of labour is that of international trade based on the international division of labour (Negishi, 1985, pp. 23–34, 1990, pp. 95 103; Blaug, 1985, p. 61; Baranzini and Scazzieri, 1990).

Ricardo

From the point of view of the standard two-commodity two-factor two-country model of the neo-classical theory of international trade, the model of Ricardo's numerical example of comparative advantage has been regarded as its special case where labour is the single factor of production. Since England's labour productivity is lower than Portugal's in Ricardo's example, however, this implies strangely that the wage rate which should be at the subsistence level is lower in the advanced country England than in the less advanced Portugal. I challenged this neo-classical interpretation of Ricardo in *History of Political Economy* (Negishi, 1982a) and argued that there exist labour, capital and land in Ricardo's model. Since England has more capital and labour relative to land, the marginal labour productivity is lower even if England's technology is not less advanced than that of Portugal. If England's subsistence level is not lower than Portugal's, furthermore, it is the rate of profit which is lower in England than in Portugal.

At the same time I also challenged another traditional interpretation of Ricardian theory of international trade, that it cannot determine the terms of trade unless, as was suggested by J. S. Mill, the reciprocal demand of countries is taken into consideration. Since wage costs of products are determined by the subsistence wages in Ricardo's model, the terms of trade (the relative price of exportables) can be inferred by the ratio of rates of profit. International capital mobility is implicitly assumed in classical economics, since it duly emphasized, unlike neo-classical economics, the role of exporters and importers, whose capital replaces, as Adam Smith pointed out, capitals of foreign producers as well as those of domestic ones. Capital moves from England to Portugal until the ratio of rates of profit satisfies the required risk premium for investments in foreign trade and in foreign countries. The relative price of cloth and wine is, then, determined by this risk premium which Ricardo duly emphasized, and changes in reciprocal demands for them are absorbed into changes in the labour population in two countries.

The lesson from these reinterpretations of Ricardo is that the study of the classical theory from the point of view of modern theory should not be a cutting or stretching of the former theory in a Procrustean bed of the latter. It should be a mirror in which modern theory finds the importance of what it forgot to learn from the former, in this case, the role of exporters and importers in international trade and investment (Negishi, 1982a, 1985, pp. 123–6, 1989a, pp. 131–8; Gandolfo, 1986, Vol. I, pp. 24, 28–31).

J. S. Mill

Mill's recantation of the wages fund doctrine constitutes one of the most difficult problems in the history of economics, since he did not revise his *Principles* in this respect, after he recanted the doctrine in his review of a book of W. Thornton, *On Labour* (1869). The doctrine is a simple equilibrium theory of demand and supply, that the equilibrium full employment real wage is obtained by dividing the given wages fund (e.g. the stock of wheat) by the given labour population. There is no role for labour unions, since they cannot raise the full employment real wage. Thornton attacked the equilibrium theory in general and the wages fund doctrine in particular, by using several examples. In his recantation, Mill interpreted one such example that demand cannot be increased by the reduction of the price so that there is no unique equilibrium price which equates demand with the given supply. The case suggested by Mill is conceivable in a labour market, if capitalists behave speculatively against changes in wages. Since the equilibrium wage is indeterminate, there is room for labour unions' activity to raise wages without reducing employment.

The question that remains is, then, why did Mill leave the doctrine of wages fund unchanged in his 1871 edition of his *Principles*, after he admitted that it was wrong in 1869? In my article in *History of Political Economy* (1986b), I tried to solve the question by referring to a hitherto unnoticed fact that, in the second edition of his *On Labour* (1970), Thornton denied Mill's interpretation of his example in 1869. Thornton did not assume that the demand is inelastic with respect to price in his example in 1869 and insisted not on the possibility of the indeterminacy of equilibrium price but on the fact that the bulk of the goods are sold at prices which do not equate demand and supply. In other words, Thornton had in mind, not the Walrasian *tâtonnement* process in which trade does not take place and contracts can be cancelled unless demand and supply are equalized, but the so-called non-Walrasian, non-*tâtonnement* process without recontract, in which goods are exchanged at disequilibrium prices. Certainly Mill recognized in 1871 that his interpretation of Thornton's example on which his recantation in 1869 was based was wrong and that Thornton insisted emphatically on the possibility of trades carried out in disequilibria. While Mill was ready in 1869 to admit the possibility of multiple equilibria and to deny the validity of the classical wages fund doctrine, Thornton insisted on denying it from the point of view of disequilibrium theory. To accept Thornton's suggestion was, however, impossible for Mill the equilibrium theorist, since it was a theory of an entirely different paradigm, which was immature as was pointed out in the preface to the 1871 edition of Mill's *Principles*. After the recent development of disequilibrium theory, are we ready to write Principles of Disequilibrium Economics? (Negishi, 1986b, 1989a, pp. 162–70, 181–9, 1989b; Ekelund and Thommesen, 1989; de Marchi, 1988).

Böhm-Bawerk

Böhm-Bawerk adduced three causes for the existence of interest in a stationary economy: (1) better provision for wants expected in the future than in the present; (2) undervaluation of future wants; and (3) the superiority of more roundabout methods of production. As for the last cause which is concerned with the demand for capital, Böhm-Bawerk had two different models, i.e. the model of circulating capital and the period of production, which he explained with numerical examples, and the model of fixed capital and the capital–labour ratio, for which he gave some analogies, such as a boat and net in fishing and a sewing machine in tailoring. Wicksell formulated a mathematical model based on numerical examples of the first model, and found that one equation was missing to determine the rate of interest. The missing equation should, of course, be supplied by the consideration of Böhm-Bawerk's first two causes which are concerned with the supply of capital, though this fact was not unanimously recognized until very recently. To introduce the second cause, the time preference, is to assume that people are myopic and not rational. While the second cause is an independent assumption to be added to the already assumed third cause, the first cause is considered to be generated by the third cause, since the latter can create the relative abundance of future goods compared with the present ones.

My own reconstruction of Böhm-Bawerk's theory is, therefore, to use the first cause to explain the supply of capital, and the third cause to explain the demand for capital. After some trial and error, I decided to follow Böhm-Bawerk's model of fixed capital and the capital labour ratio rather than his model of circulating capital and the period of production, since the first is more convenient to dispense with than the second cause, and the roundaboutness of production in terms of the capital labour ratio is more comfortable for neo-classical economists than that in terms of the period of production. We have to consider a life cycle model of overlapping generations of individuals which was considered by Samuelson, in which people live for finite periods, having rising consumption, and consume their lifetime incomes, to apply Böhm-Bawerk's first cause in a stationary state where an economy as a whole should be equally provided for in the future as well as in the present. Such a model is not alien, however, to Böhm-Bawerk himself. People do not save too much so that the rate of interest remains positive, not because they are myopic but because their life span is limited. Only after Samuelson, thus, can we formulate consistently what Böhm-Bawerk had in mind and see clearly the implications of the celebrated Böhm-Bawerk versus Schumpeter controversy on the rate of interest in a stationary economy (Negishi, 1985, pp. 103-8, 197, 1989a, pp. 297–317; Blaug, 1985, p. 569; Faber, 1986).

Though they are not yet recognized by others, I would like to use this occasion to emphasize the following points on Smith and Marx. The reason for my concern with them is perhaps the fact that many of my colleagues in Tokyo are Marxian.

Adam Smith (2)

The traditional interpretation of Adam Smith's value theory is that commodities exchange in proportion to embodied labour, which equals commandable labour in pre-capitalist society, while they do not exchange in proportion to the former labour, which is now smaller than the latter, in modern capitalistic society. If we read him with a growth economy in mind, however, we find that embodied and commandable labours are identical in a stationary state, whether it is a pre-capitalist society with no capital or an economy with capital where, however, the rate of profit is zero, while commandable labour is larger than the embodied one in a growing economy where the rate of profit is positive. Smith argues that in such an economy the surplus appears not only as profit in a sector where a commodity is produced from the input of labour but also in the higher natural (equilibrium) wage in a sector where labour is produced from the input of commodities. This latter surplus, which is also due to the difference between commandable and embodied labours, cannot be explained by the Ricardo–Marx theory of embodied labour value in which the natural price (value) of labour is defined so as just to reproduce labour expended in production. In other words, the embodied labour theory of value can be applied only in a stationary state.

My Smithian growth model is a von Neumann model of the production of commodities by means of commodities where commodities include labour. In the von Neumann model, all the commodity stock expands at the common constant growth rate, relative prices remain constant, and the rate of interest equals the rate of growth. In the terminology of Smith, this implies that the natural price of commodities and the natural rate of wages and profit prevail and the natural rate of wages exceeds the subsistence level by the natural rate of profit. As Smith insisted for the case of new colonies, a high natural rate of wages and that of profit coexist in an economy with high rate of growth. If we introduce saving into this model, furthermore, we can demonstrate the Malthusian regulating principle of profit that the rate of profit is determined by the demand and supply of capital, which defends Smith's theory of the falling rate of profit against Ricardo's criticism. A higher rate of saving implies the higher rate of growth but the lower rate of profit, i.e. less incentive for capitalists to produce, which was the problem of Malthus (Negishi, 1989a, pp. 83–9).

Karl Marx

Studies of Marx's economics from the point of view of modern economic theory have recently been very much developed by the application of linear economic theory. This centres, however, on the theory of value and the problem of transformation of values into prices of production. According to Marx's plan of economics, furthermore, the *Das Kapital* he left us covers only a small part of the first part of the plan, and many important problems including that of international trade are located in the second part of the plan, left to be done by his successors. In *Das Kapital*, however, Marx left some fragmentary discussions and suggestions on such problems from which we can start our own studies. In view of the North–South economic relations of the present world, what is most interesting to us may perhaps be the problem of international value, which is the foundation of Marxian theory of international trade and international exploitation.

The crux of Marxian economics is to explore the social relation between those who exploit and those who are exploited. Marx's theory does not, however, make sense so far as it is concerned with the exploitation of labour by capital, since, as was pointed out by Böhm-Bawerk, it is based on an unwarranted supposition that the labour value of an output can be compared with that of an input without any discounting, even though they are located at different times. Unless labour is mobile through time, larger value of output in terms of labour in the future compared with smaller labour value of wages paid in the past does not necessarily imply that those who advanced wage costs exploit wage earners. Marx also argued that a rich country exploits a poor one through the exchange of one day's labour of the former and three days' labour of the latter. As I argued in my presidential address to the Japan Association of Economics and Econometrics in 1985, this argument of Marx does make sense, even if labour is not mobile internationally, since in Marxian economics labour is an intermediate good produced by the consumption of consumers' goods, which are, directly or indirectly, mobile internationally, so that we can compare labours of different countries (Negishi, 1986a, 1989a, pp. 206–13).

Finally, it is perhaps my duty to emphasize that there is a great accumulation of past studies on the history of economics in Japan, though these studies are unknown in other countries, and also to develop them further in a form accessible to economists who do not understand the Japanese language. The following is an example.

Von Thünen

Studies of von Thünen in Japan have a long history. While Yasuo Kondo insisted in 1928 that von Thünen's natural wage should be the competitive equilibrium wage in a long-run stationary economy, Yuzo Yamada argued in 1934 that the natural wage and the interest rate should be determined by the macro-economic equilibrium condition of saving and investment. These studies suggest an interpretation of von Thünen's theory which is different from those of recent studies of von Thünen in the English-speaking world. A simple model of a stationary economy is constructed on the basis of such an interpretation, in which workers are assumed to be free to save in their life cycles, though the level of consumption of working families is kept at the subsistence level. Von Thünen's famous formula of the natural wage that it is the geometric mean of the marginal productivity of labour and the subsistence wage is derived from our model, which is free from the criticisms given to von Thünen's original model (Negishi, 1990).

4. *Quo vadis,* economics?

No one knows exactly in what direction economics will go in the future. The economics of the future is an unintended result, like money conceived by Menger, of the independent behaviour of many economists to change current economics in the direction they believe to be right. Being located far from the centre in which many economists are 'making modern economics', I am afraid that I am not qualified to predict the direction of the future development of economic theory. Based on what I have done and what I am doing now, however, I naturally have something to say on how economic science has to be developed. Though I said it implicitly in the above, it may be worth while recapitulating.

In whatever direction economics goes, I insist that more theoretical resources have to be mobilized to the study of its past history, if the change to that direction can ever be called progress of economic science. Progress means either to add something new and useful or to discard something old and unnecessary. To see whether we are going in the right direction, it is necessary always to see whether or not we wrongly sacrificed something useful. The history of our science can be used as a mirror in which the current theory finds the importance of what it forgot to learn from past theories. This is particularly so, since in economics something of old theories was often discarded not because of empirical refutation, but because of theoretical elegance, simplicity or symmetry. For our purpose, however, the study of the history of economics should never be an antiquarianism. It should be a comparative study of past and current theories.

As we saw in section 3, for example, a lesson from the study of Ricardo's theory is the role of exporters and importers in international trade and investment, which modern theory did not inherit from classical theory. This is not independent of the fact that capital is mainly considered as circulating capital like the advancement of wage cost in classical economics, while it is typically considered as fixed capital like machines in neo-classical economics, since the role of exporters and importers in the former is to replace capital advanced by domestic and foreign producers. In the neo-classical theory of production theory, thus, capital, labour and land can be treated symmetrically. The neo-classical theory simplifies international trade by assuming that consumers can buy in foreign as well as in domestic markets, so that it can discard the exporters and importers from its elegant theoretical models. So far as all the prices are flexible and all the factors of production can be fully employed, it can be said that economics has progressed from the classical to the neo-classical theory, since exporters and importers can be safely assumed away and the latter theory can produce many clearer results from simpler models.

Let us consider Keynesian situations, however, by introducing price rigidity, which may be explained, as we did in section 2, by kinked demand curves perceived by firms. The import demand of a country is, then, a function of the rate of foreign exchange, since consumers of the country can spend their income given in terms of the domestic currency on foreign goods with prices given in terms of foreign currency. An appreciation of the domestic currency should theoretically increase the import demand, which contradicts, unfortunately, our recent experiences. If we consider that domestic consumers buy foreign goods only in domestic markets from domestic importers or foreign exporters, however, the inelasticity of import demand with respect to the rate of exchange can easily be explained. There is no price effect of the appreciation of domestic currency on import demand, since the prices of foreign goods are given in terms of domestic currency. The only conceivable effects are income effects of increases of profit of domestic importers, which are roughly cancelled out by those of decreased profit of domestic exporters (see Itoh and Negishi, 1987, pp. 89–93; Negishi, 1985, pp. 126–31).

Thus, it is clear that it really was a useful simplification in the neo-classical theory of international trade to assume away the role of exporters and importers, which was firmly recognized in classical theory. The comparative studies of past and current theories are always necessary for economic science to progress in the right direction.

Notes

1. This is, of course, the story of an economist who belongs to the last generation of those who were educated before the recent rapid growth of the Japanese economy. Nowadays, Japanese economists are convinced that their economy is as mature as those of Western advanced countries. Now Japanese students are taught by standard textbooks of micro- and macro-economics. The young generation of Japanese economists are trying to develop Japanese theory based on the Japanese economy.
2. Another problem I studied in the 1970s is that of public economics. See Negishi (1979, pp. 167–203).
3. Since I was elected Dean of the Faculty of Economics in 1990, further research activity has had to be suspended.
4. See Negishi (1962, 1972, pp. 191–206). I was much influenced by Allais (1943, vol. 2, pp. 486–9).
5. See Negishi (1962, 1972, pp. 207–27), Hahn and Negishi (1962) and Negishi (1979, pp. 9–25). Subsequently, non-*tâtonnement* models with production have been extensively studied by Fisher (1983).
6. Other contributions in international economics are problems of gains from trade (see Kemp, 1969a, pp. 274, 281, 288; Kemp and Negishi, 1970; Negishi, 1972, pp. 73–89; Corden, 1984; Helpman 1984); domestic distortions (see Kemp and Negishi, 1969; Negishi, 1972, pp. 158–78; Bhagwati, 1969, pp. 295–308; Smith, 1977; and imperfect competition (see Negishi, 1972, pp. 116–27; Krugman, 1979; Gandolfo, 1986, pp. 54, 75).
7. For quantity constraint models, see Benassy (1978, 1982, pp. 28–40, 61–70) and Malinvaud (1985, pp. 1–80).
8. For SRP, see, for example, Boland (1987).
9. It is interesting to see that more than half the graduate students in US elite schools are moderately interested in the history of economics, though only less than 20 per cent of them have great interest. See Klamer and Colander (1990, p. 17).
10. Besides those which follow, see Negishi (1982b, 1989a, pp. 319–44, 363–75), Ekelund and Shieh (1989), and Groenewegen (1990) for my studies on Jevons and Marshall.

References

Allais, M. (1943) *Traité d'économie pure.* Paris: Imprimerie Nationale.

Arrow, K. J. and Hahn, F. H. (1971) *General Competitive Analysis.* San Francisco: Holden-Day.

Baranzini, M. and Scazzieri, R. (1990) Economic structure: analytical perspectives. In M. Baranzini and R. Scazzieri (eds), *The Economic Theory of Structure and Change.* Cambridge: Cambridge University Press, 227–333.

Benassy, J.-P. (1978) A neo-Keynesian model of price and quantity determination in disequilibrium. In G. Schwödianer (ed), *Equilibrium and Disequilibrium in Economic Theory.* Dordrecht: D. Reidel, 511–44.

Benassy, J.-P. (1982) *The Economics of Market Disequilibrium.* New York: Academic Press.

Bhagwati, J. N. (1969) *Trade, Tariffs and Growth.* London: Weidenfeld and Nicolson, 295–308.

Blaug, M. (1985) *Economic Theory in Retrospect.* Cambridge: Cambridge University Press.

Boland, L. A. (1987) On the relevance of neo-Walrasian economic theory: a review. *History of Political Economy*, **19**, 659–66.

Corden, W. M. (1974) *Trade Policy and Economic Welfare.* Oxford: Oxford University Press, 256–7.

Corden, W. M. (1984) The normative theory of international trade. In R. W. Jones and P. B. Kenen (eds), *Handbook of International Economics*, Vol. I. Amsterdam: North-Holland, 63–130.

Cuddington, J. T., Johansson, P.-O. and Löfgren, K.-G. (1984) *Disequilibrium Macroeconomics in Open Economies.* Oxford: Blackwell, 40–4.

De Marchi, N. (1988) John Stuart Mill interpretation since Schumpeter. In W. O. Thweatt (ed), *Classical Political Economy.* Boston: Kluwer, 137–62.

Diewert, W. E. (1970) On a theorem of Negishi. *Metroeconomica*, **25**, 119–35.

Ekelund, R. B. Jr and Shieh, Y.-N. (1989) Jevons on utility, exchange and demand theory: a reassessment. *Manchester School*, **57**, 17–33.

Ekelund, R. B. Jr and Thommesen, S. (1989) Disequilibrium theory and Thornton's assault on the laws of supply and demand. *History of Political Economy*, **21**, 567–92.

Faber, M. (1986) On the development of Austrian capital theory. In M. Faber (ed), *Studies in Austrian Capital Theory, Investment and Time.* Berlin: Springer-Verlag, 12–43.

Fisher, F. M. (1983) *Disequilibrium Foundations of Equilibrium Economics.* Cambridge: Cambridge University Press.

Gandolfo, G. (1986) *International Economics.* Berlin: Springer-Verlag.

Ginsburgh, V. and Waelbroeck, J. (1976) Computational experience with a large general equilibrium model. In J. Los and M. W. Los (eds), *Computing Equilibria.* Amsterdam: North-Holland, 257–69.

Groenewegen, P. (1990) Neoclassical value and distribution theory: the English speaking pioneers. In K. Hennings and W. Samuels (eds), *Neoclassical Economic Theory, 1870–1930.* Boston: Kluwer, 13–51.

Hahn, F. H. (1977) Keynesian economics and general equilibrium theory: reflections on some current debates. In G. C. Harcourt (ed), *The Microeconomic Foundations of Macroeconomics.* London: Macmillan, 25–40.

Hahn, F. H. (1990) Expectations. In J. D. Hay and D. Winch (eds), *A Century of Economics*, Oxford: Blackwell, 232–60.

Hahn, F. H. and Negishi, T. (1962) A theorem on non-*tâtonnement* stability. *Econometrica*, **30**, 463–9.

Hart, O. D. (1984) Imperfect competition in general equilibrium: an overview of recent work. In K. J. Arrow and S. Honkapohyja (eds), *Frontiers of Economics*. Oxford: Blackwell, 101–49.

Helpman, E. (1984) Increasing returns, imperfect markets, and trade theory. In R. W. Jones and P. B. Kenen (eds), *Handbook of International Economics*, Vol. I. Amsterdam: North-Holland, 325–65.

Itoh, M. and Negishi, T. (1987) *Disequilibrium Trade Theories*. Chur: Harwood.

Kemp, M. C. (1969a) *The Pure Theory of International Trade and Investment*. Englewood Cliffs, NJ: Prentice Hall.

Kemp, M. C. (1969b) *A Contribution to the General Equilibrium Theory of Preferential Trading*. Amsterdam: North-Holland, 8, 148.

Kemp, M. C. and Negishi, T. (1969) Domestic distortions, tariffs, and the theory of optimum subsidy. *Journal of Political Economy*, **77**, 1011–13.

Kemp, M. C. and Negishi, T. (1970) Variable returns to scale, commodity taxes, factor market distortions, and their implications for trade gains. *Swedish Journal of Economics*, **72**, 1–11.

Keynes, J. M. (1936) *The General Theory of Employment, Interest and Money*. London: Macmillan.

Klamer, A. and Colander, D. (1990) *The Making of an Economist*. Boulder: Westview Press.

Krugman, P. R. (1979) Increasing returns, monopolistic competition, and international trade. *Journal of International Economics*, **9**, 469–79.

Malinvaud, E. (1985) *The Theory of Unemployment Reconsidered*. Oxford: Blackwell.

Mill, J.S. (1871) *Principles of Political Economy*. London: Longmans, Green.

Negishi, T. (1960) Welfare economics and the existence of an equilibrium for a competitive economy. *Metroeconomica*, **12**, 92–7.

Negishi, T. (1960–61) Monopolistic competition and general equilibrium. *Review of Economic Studies*, **28**, 196–201.

Negishi, T. (1962) The stability of a competitive economy: a survey article. *Econometrica*, **30**, 635–69.

Negishi, T. (1968) Protection of the infant industry and dynamic internal economies. *Economic Record*, **44**, 56–67.

Negishi, T. (1969) The customs union and the theory of second best. *International Economic Review*, **10**, 391–8.

Negishi, T. (1972) *General Equilibrium Theory and International Trade*. Amsterdam: North-Holland.

Negishi, T. (1974) Involuntary unemployment and market imperfection. *Economic Studies Quarterly*, **25**(1), 32–46.

Negishi, T. (1979) *Microeconomic Foundation of Keynesian Macroeconomics*. Amsterdam: North-Holland.

Negishi, T. (1982a) The labor theory of value in the Ricardian theory of international trade. *History of Political Economy*, **14**, 199–210.

Negishi, T. (1982b) A note on Jevons's law of indifference and competitive equilibrium. *Manchester School*, **50**, 220–30.

Negishi, T. (1985). *Economic Theories in a Non-Walrasian Tradition*. Cambridge: Cambridge University Press.

Negishi, T. (1986a) Marx and Böhm-Bawerk. *Economic Studies Quarterly*, **37**, 2–10.

Negishi, T. (1986b) Thornton's criticism of equilibrium theory and Mill. *History of Political Economy*, **18**, 567–77.

Negishi, T. (1989a) *History of Economic Theory*. Amsterdam: North-Holland.

Negishi, T. (1989b) On equilibrium and disequilibrium: a reply to Ekelund and Thommesen. *History of Political Economy*, **21**, 593–600.

Negishi, T. (1990) Studies of von Thünen in Japan. *Japan and the World Economy*, **2**, 199–209.

Reid, G. C. (1981) *The Kinked Demand Curve Analysis of Oligopoly: theory and evidence*. Edinburgh: Edinburgh University Press.

Smith, M. A. M. (1977) Capital accumulation in the open two-sector economy. *Economic Journal*, **87**, 273–82.

Thornton, W. H. (1869, 1970) *On Labour*. London: Macmillan.

3

Ariel Rubinstein

A subjective perspective on the interpretation of economic theory

In keeping with the general framework of this book, I will confine myself to a short summary of my own subjective perspective on economic theory. It is a perspective that began to take shape in my undergraduate days at the Hebrew University of Jerusalem, where I started my academic life as a student of mathematics. The university had a programme called 'Mathematics and combinations' which allowed us, besides concentrating on mathematics, to take courses in economics or any other academic field of our choosing. The combination of mathematics and economics was quite rare. As a group of students enrolled in this programme, we found ourselves fascinated by the sharp intellect and brightness of our mathematics teachers. The economics courses were easy and boring in comparison. We felt economics was intellectually inferior to the 'queen of sciences'. We were not, however, mathematics fanatics. In the cafeteria, where we spent many happy afternoons engaged in discussion, we found ourselves departing from the mathematical symbols in the search for metaphors for the highly abstract topological and measure-theoretical concepts. We would attempt to give these notions a real-life verbal interpretation. At the time we did not know of any mathematical model in the social sciences, and economics was still a combination of boring verbal material and unbearable calculations of derivatives. We felt intuitively, however, that something lay beyond the symbols.

On graduating to more advanced courses we were fortunate to meet two great economists, Bob Aumann and Menachem Yaari. Both these scholars proved to be thorough thinkers and firmly believed in the value of interpreting models. They provided us with standards for using mathematical tools in

---

The editor, A. Heertje, provided the encouragement for this essay. I am grateful to David Cooper for his valuable comments.

economics, and used mathematics rigorously. They insisted that definitions be well defined and proofs properly proved. By changing words for mathematical symbols we basically distance ourselves from the layman. Such a move can be justified if we insist that mathematics be used to ensure accuracy. Aumann and Yaari taught us that in the absence of a proper set-up for the model, these objectives cannot be achieved. Many economists still reject this view and regard such requirements as a mathematical conspiracy. The use of mathematics, however, is not necessarily beneficial. As a former student of mathematics I myself was guilty, being infatuated with mathematical thoroughness as a criterion for good economic theory. Occasionally a theorem is responsible for a surprising discovery but often it helps cover up a deficient 'economic content'.

The power of elementary mathematics first struck me in a second-year undergraduate course in mathematical logic. I enjoyed the course more than any other I have ever taken and owe much of my perspective on economic theory to it. The teacher, Saaron Shelach, made the course so difficult and intensive that it was all I could do to copy notes from the blackboard during class. However, the long days and nights I spent trying to decipher the symbols and poring over the challenging homework remain among the happiest moments of my academic life. I was fascinated by the simplicity and ingenuity of the definitions and the strength of the conclusions. There was a sense of 'touching the heart' of human reasoning.

Today, 12 years after finishing my PhD, I am still puzzled by the question of how mathematical theorems are relevant to real-life reasoning. This topic is probably the most interesting in economic theory. Notwithstanding certain doubts, I am still fascinated by the same question that occupied and enlightened me during my happy undergraduate days. This question concerns the almost magical connection between the formal definitions and theorems on the one hand and statements of natural language on the other hand. I find both the search for the interpretations and that for the correct language of economic theory very exciting. Indeed, these issues constitute the main drive of my personal research.

The key word in human behaviour is in my opinion the word 'language' and this chapter is built around this word. The chapter is a collection of independent comments organized as follows:

1. *Language and decision theory.* In the first part (based on Rubinstein, 1978) it is argued that decision-making language should be a part of decision theory.
2. *Language for stating theorems about theorems.* Mathematical logic deals with language in which theorems are stated, and this part (based on Rubinstein, 1984) is a demonstration of the possibilities of using mathematical logic methods to prove theorems about theorems.
3. *The language of bargaining theory: preferences versus utilities.* In this part (based on Rubinstein, Safra and Thomson, 1990) bargaining theory is used to demonstrate the importance of the right choice of language when stating a theory.

4. *The choice of names for concepts: what is a 'strategy'?* In this part (based on Rubinstein, 1991) it is argued through discussion of the concept of 'strategy' in an extensive game, that the choice of name applied to a concept can lead to serious difficulties.
5. *The interpretation of equilibrium strategies in sequential games.* Finally, it is argued that the interest in game-theoretic results lies in the possibility of interpreting the strategies and that 'automata' are a useful tool in the rigorous interpretation of a strategy in sequential games. This part uses ideas from Rubinstein (1986) and Rubinstein and Wolinsky (1990).

1. Language and decision theory

Decision theory is the most primitive theory in which the word 'language' can be expected to appear. In most economic models, a decision-maker is described simply by a preference relation (or equivalently a utility function). We permit preferences to be quite unrestricted, and in particular we allow them to be non-verbal. Rubinstein (1978, p. 2) tries 'to exemplify the importance of including the element of language in discussions within the theories of social choice, utility and measurement'. My argument was that: 'We instinctively justify to ourselves any decision and try to rationalize it. We are inclined to formulate judgments in words and to justify them in words' (p. 16). The need to define a preference in words is natural in cases where the decision-maker is a group of agents and decision-making involves communication among members of the group. Even when the decision-maker is an individual, he tends to justify his behaviour by giving reasons so that the preferences must be expressible in some kind of language. We must therefore look at the implications of the restrictions imposed by language on the set of relevant preferences. Whether something is 'definable' depends on the decision-maker's language and therefore the study of the connection between language and the admissible preferences should be at the heart of decision theory.

The following is one of the three examples which were presented in Rubinstein (1978). A youth arrives in a strange town. He is about to receive two offers of friendship. The information he receives about each of the girls consists of a list of boys she has dated over the last n days. Given that he is a stranger in town, let us assume that all he can glean from any two given names on the list is whether they are identical. This language is called the 'pure language with equality'. The atomic formulae in this language are formulae of the type $z_1 = z_2$, where z_1 and z_2 are symbols from among $\{x_1, \ldots, x_n, y_1, \ldots, y_n\}$. A formula is either an atomic formula or a string of symbols which is constructed inductively by the rules: if ϕ and ψ are formulae then '$-\phi$', 'ϕ and ψ', 'ϕ or ψ', '$\forall\phi(x)$', and '$\exists\phi(x)$' are all formulae as well.

What preferences can be stated in this simple language? That is, which formulae can induce a preference suitable for all possible 'worlds' in which our stranger is likely to find himself? Or formally, what are the definable preferences where definability of the preference is taken as the existence of a formula $\phi(x_1, \dots, x_n, y_1, \dots, y_n)$ so that the stranger prefers the girl with the list $(a_1, \dots, a_n)$ over the girl with the list $(b_1, \dots, b_n)$ iff the formula ϕ is true where each variable x_i is replaced by a_i and each variable y_i is replaced by b_i. To answer this question, define $E(a_1, \dots, a_n)$ to be the partition of $\{1, \dots, n\}$ in which i and j are in the same partition iff $a_i = a_j$. Thus, if the girl is unstable in her relationships and all a_i are distinct $E(a_1, \dots, a_n)$ is the finest partition $\{\{a_1\}, \dots, \{a_n\}\}$ and if she is very stable and all a_i are identical then $E(a_1, \dots, a_n)$ is the coarsest partition $\{\{a_1, \dots, a_n\}\}$. It is not difficult to verify that the only definable preferences are those which are induced by orderings of partitions of $\{1, \dots, n\}$, so that

$$(a_1, \dots, a_n) > (b_1, \dots, b_n)$$

if $E(a_1, \dots, a_n)$ is preferred to $E(b_1, \dots, b_n)$. In other words, any information about the equality of some a_i to b_j is ignored and only the patterns of stability matter.

The above was included in a paper which was one of my very first. The paper has never been accepted for publication, probably because the specific results were not striking and the linguistic constraints which I studied were remote from the constraints imposed by the natural language. Nevertheless, the general idea that language plays an important role in determining the decision-makers' patterns of behaviour is of interest. This idea is completely missing in current economic theory.

Notice that the decision-maker uses the 'pure language with equality' which has the remarkable property (see Robinson, 1963) that definability is equivalent to 'definability without quantifiers', i.e. every formula has an equivalent formula without quantifiers. This means that the definability in that example is equivalent to the requirement that given four vectors $(a_1, \dots, a_n)$, $(b_1, \dots, b_n)$, $(c_1, \dots, c_n)$ and $(d_1, \dots, d_n)$, we require that if for all i and j, $a_i = a_j$ iff $c_i = c_j$, $b_i = b_j$ iff $d_i = d_j$ and $a_i = b_j$ iff $c_i = d_j$ then the vector $(a_1, \dots, a_n)$ is preferred to $(b_1, \dots, b_n)$ iff $(c_1, \dots, c_n)$ is preferred to $(d_1, \dots, d_n)$. This requirement is close in spirit to the neutrality assumption so common in social choice literature.

This connection is useful for reinterpreting some of the results of social choice theory. In Arrow's framework the social ordering has to be based on the preferences of the n individuals who make up the society. Let P be the symbol for the social ordering and P_i be the symbol for individual i's preference. The social choice (strong) neutrality requirement made in social choice theory (and implied by Arrow's independence of irrelevant alternatives) states that for any two pairs of social alternatives a, b and c, d, if for all i, aP_ib iff cP_id, then aPb iff cPd. The linguistic way of expressing the axiom is that the social relation has to be 'definable without quantifiers', i.e. P should be defined by a formula without quantifiers, $\phi(x_1, x_2)$, in the language in which the atomic formulae are of the type $z_1 P_i z_2$ where the z_i are variable names. (For a formal proof of this fact see Rubinstein, 1984.) Thus, an alternative formulation of Arrow's impossibility theorem is that given a world with at least three individuals and three social alternatives, any social welfare function which satisfies definability without quantifiers and the Pareto-optimality requirements is dictatorial.

I find the linguistic interpretation of the neutrality type of axiom appealing. An even more attractive axiom is definability; a social ordering must be defined by a formula (not necessarily without quantifiers). Definability cannot replace definability without quantifiers in Arrow's theorem. Consider, for example, the social welfare function which chooses the majority rule ordering in the case where it induces an ordering or coincides with individual 1's preference otherwise. This function is not dictatorial and is defined in a language which includes the names of the individualistic preferences only. However, in the definition we must use quantifiers (in order to state the sentence 'if the majority rule induces an ordering'). To my knowledge the characterization of the definable preferences is still an open question.

2. Language for stating theorems about theorems

Economic theory may be viewed as the study of models which are used by economists: as such, it is not about economics but about models. In order to demonstrate this point let us continue the discussion of social choice theory begun in Rubinstein (1984). I view that paper as one of my better efforts and thus was frustrated by the rejection letters I received.

Social choice can be divided into multi-profile and single-profile theorems. A multi-profile theorem is about functions which assign a social ordering to every profile of preferences within a large set (all *possible societies*, not only the existing one). Such a theorem relies on a 'glue axiom', i.e. an axiom which requires dependency (or consistency) between the way that the social ordering treats a pair of alternatives in two different profiles where the two profiles treat the alternatives similarly. The most famous axiom of this sort is the independence of irrelevant alternatives. On the other hand, a single profile theorem refers to a single profile (the existing society). The aim is to attach an ordering to the society which will be sensitive to individuals' preferences. A typical axiom here is the neutrality axiom which requires that if each individual's preference over the alternatives *a* and *b* is 'the same' as his preferences between *c* and *d*, then the social ordering treats *a* and *b* like it treats *c* and *d*.

Social choice theory includes many 'single-profile analogues' to theorems which were proved first in the multi-profile framework. This motivated the following statement from Sen (1977, p. 1564): 'As a result of these important contributions it is now clear that the standard inter-profile collective choice results have exact intra-profile counterparts . . .'. Sen's conclusion is an assertion about propositions. Its formal statement requires a definition of the term 'analogue theorem'. That is where mathematical logic comes in.

Let M be the class of all possible profiles. A social function (SF) is one which assigns a binary relation to any profile in M. Let β and δ be two formulae without quantifiers in a language which includes only the symbols $P, P_1, \ldots, P_k$. Let T^* be the following theorem: if an SF satisfies strong neutrality and the proposition $\alpha = \forall v_1, \ldots, v_k \beta(v_1, \ldots, v_k)$, then it satisfies the proposition $\zeta = \forall v_1, \ldots, v_k \delta\ (v_1, \ldots, v_k)$. To identify that Arrow's impossibility theorem can be written in the scheme of T^* with $k=3$, take α to be the sentence

$$\forall v_1 v_2 v_3 \left[\left(\bigcap_{i=1,\ldots,n} v_2 P_i v_1 \rightarrow v_2 P v_1 \right) \right]$$

(which expresses Pareto-optimality) in conjunction with the requirement that P is an ordering and have

$$\beta = \bigcup_{i-1,\ldots,n} \forall v_1 v_2 (v_1 P_i v_2 \rightarrow v_1 P v_2)$$

which expresses the statement that there is a dictator.

Apparently the number k plays a critical role in the formulation of a theorem about the single-profile analogues. The single-profile analogue holds only if the single profile satisfies a 'richness property', R_k, which states: every formula without quantifiers with k variables which is satisfied by some k alternatives in some profile in M is satisfied by *some* assignment of k elements in the single profile.

Now we can state a theorem about theorems: if the theorem T^* is valid for M and if the single profile satisfies R_k (where k is the same k which appears in the statement of T^*!), then the theorem T^* is true in the single profile as well.

Rubinstein (1984) includes an example of a proposition in which α does not have the structure as it exists in T^* and for which the single-profile analogue does not exist. Thus, we conclude that the logical structure of the multi-profile proposition is a key for the existence of a single-profile analogue.

The above points to a potentially interesting line of research: searching for a formal expression of the often discussed intuitions about the connections between different models on the basis of the logical structures of the theorems in which we are interested. At the moment I am not familiar with any work in this direction.

3. The language of bargaining theory: preferences versus utilities

The language of a model affects its possible interpretations. Nash bargaining theory provides an interesting example of the effect of an unsuitable choice of primitives. The discussion in this section follows Rubinstein, Safra and Thomson (1990).

The primitives of Nash's (two-person) bargaining theory are the 'feasible set', S, and a 'disagreement point', d. Each element of S gives the utility levels reached by the two agents at one (or more) of the possible agreements. The utilities are understood to be *von Neumann–Morgenstern utilities* in that they are derived from preferences over lotteries which satisfy the expected utility assumptions. A bargaining *solution* is a function which assigns a unique pair of utility levels to each problem $\langle S, d\rangle$ taken from some domain. Nash showed that there is a unique solution satisfying the following four axioms: *invariance to positive affine transformations* (INV), *symmetry* (SYM), *Pareto-optimality* (PAR) and *independence of irrelevant alternatives* (IIA). The unique solution is the Nash solution, i.e. the function N defined by

$$N(S, d) = \arg\max\{u_1 - d_1)(u_2 - d_2) \mid (u_1, u_2) \in S \text{ and } u_i \geqslant d_i \text{ for both } i\}$$

The very simplicity of this formula is in itself an attractive feature and is responsible for the widespread application of the solution. However, my problem with the above formula is that I simply do not understand its meaning. What is a product of two von Neumann–Morgenstern utility numbers and what is the meaning of the maximization of that product?

A good bargaining solution should have an attractive verbal definition. The search for a more meaningful definition of the Nash solution leads to a switch from utility language to alternatives-preferences language. A Nash problem, $\langle S, d\rangle$, is replaced by $\langle X, D, \geqslant_1, \geqslant_2\rangle$ where X is a set of feasible (deterministic) alternatives described in physical terms, D is the disagreement alternative and $\geqslant_1$ and $\geqslant_2$ are preferences defined on the space of lotteries in which the prizes are D and the elements of X.

Recall that we are looking for an alternative definition of the Nash solution, one which only uses the terms 'alternative', 'disagreement' and 'preference' and which avoids the term 'utility'. We look for an explicit definition which specifies the outcome of a particular problem directly in terms of the problem without referring to consistency with the outcomes suggested by the solution of other problems.

For the alternative definition, denote by px the lottery which gives x with probability p and D with probability $1 - p$.

DEFINITION An (ordinal)-Nash solution outcome for the problem $\langle X, D, \geqslant_1, \geqslant_2\rangle$ is an alternative y^* such that for all $p \in [0,1]$ and for all $x \in X$ and i, if $px >_i y^*$ then $py^* \geqslant_j x$.

Thus we interpret the solution as a convention which assigns to every bargaining problem an outcome with the following property: assume that the players perceive that whenever they raise an objection to an alternative, they face a risk that the negotiations will end in disagreement. If it is worth while for one of the players to make a demand for an improvement upon the convention, which may cause a breakdown of the negotiations, then it is optimal for the other player to reject the demand and to insist on following the convention even when taking into account the possibility of negotiations breaking down.

It is interesting to note that the above definition is close in spirit to an idea suggested by Zeuthen (1930) who was the first to build a theory in which negotiators bear in mind the risk of a breakdown in negotiations.

The first thing to notice about the ordinal definition is that for the expected utility case it coincides with the definition of the (utility)-Nash solution. Basically, it follows from the fact that the alternative y^* satisfies

$$u_1(y^*)u_2(y^*) \geqslant u_1(x)u_2(x) \text{ for all } x \text{ for both } i$$

if and only if

for both i, for all $x >_i y^*$ for all $p \leqslant 1$, if $p > u_i(y^*)/u_i(x)$ then $p \geqslant u_j(x)/u_j(y^*)$

if and only if

for all i and x, $px >_i y^*$ implies $py^* \geqslant_i x$

The switch to the alternatives-preferences language allows a restatement of the entire Nash theory. In particular, it is possible to translate the axioms into more natural language and to derive the Nash characterization theorem. As long as we fix the set of alternatives (and only allow a variation of the preferences) the Invariance of Affine Transformations (IAT) axiom is equivalent to the requirement that rescaling of the utilities should not affect the alternative predicted by the solution. Once we switch to the alternatives-preferences language, IAT becomes redundant. Nash's axioms PAR and SYM can easily be translated. The main difficulty is to restate IIA: if a^* is the solution outcome of the problem $\langle T, d\rangle$ and is a member of a set S which is a subset of T, then a^* is also the solution outcome of $\langle S, d\rangle$. As has often been emphasized, this justification of IIA fits in with a normative theory, where the solution concept is intended to reflect the social desirability of an alternative. When bargaining is viewed as a strategic interaction of self-interested bargainers the IIA axiom is questionable. The following is an alternative statement of IIA which does not require a comparison between problems with different sets of alternatives (we use the letter F to denote a solution):

IIA: Let $F(\geqslant_1, \geqslant_2) = y^*$ and let $\geqslant'_i$ be a preference which agrees with $\geqslant_i$ on the set of deterministic agreements, X, such that:

1. For all x such that $x \geqslant_i y^*$, if $px \sim_i y^*$ then $px \leqslant'_i y^*$.
2. For all x such that $x \leqslant_i y^*$, if $x \sim_i qy^*$ then $x \sim'_i qy^*$.

Then $F(\geqslant_i, \geqslant_j) = F(\geqslant'_i, \geqslant_j)$.

The switch of agent i's preference from $\geqslant_i$ to $\geqslant'_i$ reflects his increased apprehension towards the risk of demanding alternatives which are *better* than the outcome y^*. Though player i still prefers x to y^* he is less willing to risk demanding x. The axiom captures an intuition that the bargaining solution outcome y^* should be defendable against possible objections. The change in player i's preference, which is described in the axiom, makes player i 'less eager' to object and does not change the intensity of player j's objections. Thus, the change in the preference 'should not' change the bargaining outcome.

The ordinal definition allows us to extend Nash theory to preferences beyond those which satisfy the expected utility theory. In recent years there has been a growing interest in non-expected utility theories of decision-making under uncertainty since they explain a wide range of behaviour patterns and experimental results that are inconsistent with expected utility theory (see Machina, 1987). Extending Nash (1950) we could show that for a certain class of preferences (which includes all expected utility preferences) the Nash solution is the unique solution which satisfies the axioms PAR, SYM and IIA. A by-product of the definition is a better understanding of classical results such as the proposition that the more risk-averse the player, the worse his outcome in the bargaining.

Another by-product of the definition is a better understanding of the connection between Nash's theory and the strategic alternating offers model (see Rubinstein, 1982). The existence of a connection between these two models was first cited by Binmore (1987). When Binmore presented his result it was like a puzzle. Whereas the Nash bargaining theory reflects the attitude of the players towards risk, Rubinstein (1982) dealt with time preferences. However, in Binmore, Rubinstein and Wolinsky (1986) we equalized the primitives by looking at a version of the infinite alternating offers model where the players do not have time preferences but at the end of each period there is a probability $1 - p > 0$ of breakdown. For the expected utility case the model has a unique subgame perfect equilibrium characterized by two alternatives $x^*(p)$ and $y^*(p)$ satisfying $px^*(p) \sim_1 y^*(p)$ and $py^*(p) \sim_2 x^*(p)$. Player 1 always offers $x^*(p)$ and accepts any alternative y such that $y \succcurlyeq_1 y^*(p)$, while player 2 always offers $y^*(p)$ and accepts any alternative x such that $x \succcurlyeq_2 x^*(p)$.

Given the ordinal Nash bargaining solution, the connection between the alternating offers equilibrium outcomes, $x^*(p)$ and $y^*(p)$, and $N(\succcurlyeq_1, \succcurlyeq_2)$ is clear: where $p \to 1$, both $x^*(p)$ and $y^*(p)$ converge to $N(\succcurlyeq_1, \succcurlyeq_2)$.

To summarize, the utility language allows the use of geometrical presentations and facilitates analysis; however, the parametric presentation results in an unnatural statement of the solution and axioms and the judgement and interpretation of the axioms and bargaining solutions are made more difficult. The difficulties are even more severe when 'technical' assumptions (such as continuity and differentiability) are made. The switch to the alternatives-preferences language allows a more natural statement of the Nash solution. It enables us to extend the definition to non-expected utility preferences and helps us to understand better certain well-known results. Making this switch in other areas of economics and game theory may also prove beneficial.

4. The choice of names for concepts: what is a 'strategy'?

Choosing a name for a technical term satisfies our desire to link economic theory with the real world. However, it also creates the risk that a particular interpretation which is correct in one context will be incorrectly applied in another. An example of such a case is the use of the word 'strategy' in game theory. Rubinstein (1991) discusses the issue more generally; here I make do with a summary of the argument for extensive games.

A strategy in game theory is usually interpreted as 'a plan of action', 'a complete description of how a player intends to play a game, from beginning to end' or 'a set of instructions'. This interpretation is consistent with the use of the term 'pure strategy' in a normal form game. However, is it appropriate in the context of extensive games? In an extensive game, a player's strategy is required to specify an action for each node in the game tree at which the player has to move. Accordingly, a player has to specify an action for every sequence of events which is consistent with the rules of the game. In games which require a player to make at least two consecutive moves (and most of the games which have been analysed recently in economic theory fall into this category), a strategy must have its actions specified even after histories which are inconsistent with the player's own strategy. For illustration, consider the following two-player game form:

```
       C       C       C
  1 ------ 2 ------ 1 -----
  |S       |S       |S
```

According to the natural definition of 'strategy' as a complete 'plan of action', player 1 is required to specify his behaviour, 'Continue' or 'Stop', at the initial node and, if he plans to 'Continue', to make provisional plans for his second decision node in the event that player 2 chooses C. However, the game-theoretic definition of strategy requires player 1 to specify his action at the second decision node, even if he plans to 'Stop' the game at the first node.

Why does the notion of strategy as used by game theorists differ from a 'plan of action'? If we were only investigating Nash equilibria of extensive games, then the game-theoretic definition would indeed be unnecessarily broad. The broad definition is, however, necessary for testing the rationality of a player's plan, both at the beginning of the game and at the point where he must consider the possibility of response to an opponent's potential deviation (the subgame perfect idea). Returning to the example above, assume that each player plans to choose 'Stop' at his first decision node. Testing the optimality of player 2's plan following player 1's deviation, requires player 2 to specify his expectations regarding player 1's plan at his second decision node. The specification of player 1's action after both players have chosen C provides these expectations and has to be interpreted as what would be player 2's (as opposed to player 1's) *belief* regarding player 1's planned future play, should player 1 decide to deviate from what was believed to be his original plan of action. Thus, a strategy encompasses not only the player's plan but also his opponent's expectations in the event that he does not follow that plan. Thus, an equilibrium strategy describes a player's plan of action, as well as those considerations which support the optimality of his plan (i.e. preconceived ideas concerning the other player's plans) rather than being merely a description of a 'plan of action'.

Interpreting a player's strategy after a deviation as the expectations of the other players about his future behaviour, makes it problematic to speak of a 'choice of strategy'. Player 1 does not choose player 2's belief. This observation has a serious impact on many of the game-theoretic assumptions. Consider, for example, the sequential bargaining literature in which the authors *assume* that strategies are stationary in the sense that a player's offer and response (to offers made by the other player) must be independent of the history of the game. This literature presents stationarity as an assumption of simplicity of behaviour. Consider, for example, player 1's strategy: 'Demand 50 per cent of the surplus and reject any offer which gives you less than 50 per cent, independent of what has happened in the past.' This strategy is simple in the sense that player 1 plans to make the same offer and make the same responses independently of how player 2 has reacted in the past. However, this strategy also implies that player 2 believes that player 1 would demand 50 per cent of the surplus even if player 1 demanded 60 per cent of the surplus in the first, let us say, 17 periods of bargaining. Thus, stationarity, as stated in sequential bargaining theory, means not only simplicity but also passivity of beliefs. This is strange, especially if we assume simplicity of behaviour. If player 2 believes that player 1 is constrained to choose a stationary plan of action, then player 2 should believe (after 17 repetitions of the demand of 60 per cent) that player 1 will continue to demand 60 percent. Thus, assuming passivity of beliefs eliminates a great deal of what sequential games are intended to model, namely, the changing pattern in players' behaviour and beliefs, as they accumulate experience.

5. The interpretation of equilibrium strategies in sequential games

Many of the major contributions to economics and game theory are concerned with types of games which have similar structures to repeated games. Much of the research in this area aims to prove what is called 'folk theorems'. This (inappropriate) name is given to theorems which state that under certain conditions, nearly all reasonable pay-off vectors can be sustained as equilibrium pay-off vectors. Thus, according to these theorems, pay-off vectors which are socially desirable can be maintained in equilibrium if the players have in mind long-term considerations; however, the model lacks predictive power.

In my opinion, the main achievement of these models is in clarifying the logic behind social institutions associated with interactions over the long term.

Characterizing the exact set of equilibrium pay-off vectors is less exciting than discussing the equilibrium *strategies*, since the verbal content of the equilibrium lies in the strategies and not in the pay-off vectors. Most literature on repeated games deals with characterizing the set of equilibrium *pay-off* vectors while the plausibility of the strategies is largely ignored. Existence theorems demonstrate unintuitive strategies. For example, the folk theorem concerning the limit of the means is sometimes proved using equilibrium in which a deviation at the nth period is met with punishment for n^2 periods. This is done for the convenience of proving the folk theorem in its maximal range. Or, in other examples, folk theorems often use equilibria which carry the element of 'punishing the punisher for not punishing', i.e. if player i 'deserves punishment' and player j does not punish player i, then player i is supposed to punish player j for not punishing him. Though I suppose there are some bizarre scenarios in which a criminal sues a policeman for not punishing him as severely as he should have, I doubt that this is a common mode of behaviour.

I myself am not blameless when it comes to proving folk theorems and ignoring the plausibility of the strategies. In my early papers on repeated games with the limit of the means (Rubinstein, 1977) and on the overtaking criterion (Rubinstein, 1979b) I did not pay enough attention to this issue. In retrospect, I believe that in order to clarify the nature of long-term interactions, we must deal with the equilibrium strategies' schemata. When discussing a scheme of strategy I am referring to its structure, stripped of the details which arise from a particular pay-off matrix. Accordingly, the value of the folk theorems is their usefulness in clarifying the rationale and credibility of codes of behaviour in which a deviator is punished for a finite number of periods before the world returns to routine behaviour.

Let me expand the discussion using two of my papers which are concerned more with the structure of strategies than with the outcome in terms of 'pay-offs': Rubinstein (1979a) is a study of the following problem: player 1 monitors player 2's behaviour. At every period player 2 can choose one of two actions, 'G' or 'B'. The B-action results (with probability 1) in a harmful accident while the G-action causes an accident only with probability $1 > p > 0$. In any particular period, player 1 has the means to punish player 2 severely enough to deter him from behaving 'badly'. However, the punishment is 'costly' for both players and player 1 looks for schemes of behaviour in which he will be able to deter player 2 effectively and cheaply. The problem can be viewed as a leader follower situation in which player 1 leads by making a binding announcement which specifies the histories, following which player 1 punishes player 2, and player 2 responds optimally by choosing a strategy which specifies, for all sequences of events, whether he will choose 'G' or 'B'. Both players are assumed to maximize their expected 'limit of the averages' pay-offs.

The leader's dilemma is quite clear. On the one hand he wants to avoid punishing player 2 too often even if he sticks to the good behaviour mode. On the other hand, he wants to avoid player 2 taking advantage of his leniency.

The main idea of Rubinstein (1979a) (independently proposed by Radner, 1981, as well) is that player 1 can achieve his first best result by employing a strategy in which he behaves as a statistician, i.e. he keeps track of the frequency of accidents and punishes player 2 whenever the frequency exceeds $p+\alpha_t$ where $(\alpha_t)_{t=1,2,\ldots}$, is a sequence of positive numbers which is selected carefully to satisfy two conditions: (1) it converges to zero so that the follower cannot achieve a positive frequency of being 'Bad' without being punished, and (2) the convergence to zero is slow enough to allow player 1 to tolerate some of player 2's 'bad luck' accidents. The law of iterated logarithm guarantees that such a sequence exists.

In the meantime, many other strategies have been suggested in the literature to solve the leader's problem. In some, the mathematics which is required to prove their effectiveness calls for theorems which are more elementary than the law of iterated logarithm. Others rely on 'dynamic programming' techniques (see Abreu, Pearce and Staccheti, 1986, and for a survey see Pearce, 1991). The basic idea of these equilibria is the following: the equilibrium is built around two 'phases', 'A' and 'B'; in phase A the leader ignores the accidents and in phase B he does not. The switch from one phase to another is stochastic and its probabilities depend on the outcome of the one-shot situation so that the follower is motivated always to choose 'G' when he solves the 'short-term' problem in which the probabilities of continuation are determined by the transition probabilities. Personally, I find the first mechanism more plausible since it captures a type of reasoning which we observe and use in real life.

A second example is taken from Rubinstein and Wolinsky (1990). Consider a market with an indivisible good, one seller and $B > 1$ potential buyers. All buyers have an identical reservation value of 1, while the seller's reservation value is zero. The competitive equilibrium price is 1. Asher Wolinsky and my aim was to examine whether we get the competitive outcome in models with pairwise matching and bargaining. To do this, we add details about the matching and bargaining processes to the basic situation. In each period the seller is matched randomly with one buyer, and one of the two (picked with equal probability) has to make a price offer which the other either accepts or rejects. The process repeats itself until an acceptance is made. The natural competitive forces are supposed to kick in here since the buyer is under the risk of losing the potential partner while the seller is not. Indeed, the model has an equilibrium with the competitive price in which the good is sold to the first buyer which the seller meets. In that equilibrium the seller always makes the request for price 1 and rejects any lower offer. However, the model has other equilibria in which non-competitive prices prevail. Let p^* be an arbitrary price, let us say the 'fair' price according to some ethical evaluation. To defend this price we introduce the institution of the 'right' to purchase at p^*: at the beginning of the game the right is granted to one of the players i^*. The right to purchase the good for p^* is kept by the right holder until either: (1) the seller approaches buyer j with an offer above p^*, in which case the right is transferred to j (in order to neutralize the competitive forces); or (2) buyer j approaches the seller with an offer above p^* in which case the players move to the competitive regime and behave as in the equilibrium which supports price 1. In the jargon of repeated games theory, the change in the regime means that the seller punishes the buyer who deviates from the equilibrium, although he offers the seller a price higher than he would otherwise get. This is an unacceptable interpretation. We should interpret the switch as a change in the market state from a mode in which a right is given to one of the buyers to a mode which gives the competitive state. The attempt of a buyer to pay a price above p^* is taken by the agents to mean that there are two serious buyers in the market who are ready to compete and this pushes the price up to the competitive level.

In reality we do observe 'privileges' in trade. However, I am not aware of any case in which the rules of transferring a right include a provision that the right is transferred when the seller approaches another buyer. Still, I find the above equilibrium attractive as it is easily described verbally and includes components of familiar institutions.

The effort to interpret sequential games equilibrium strategies leads to the search for a formal model in which we can refer to the interpretation of the strategies more rigorously. This was part of my motivation in Rubinstein (1986) where I replaced the strategies in repeated games with a machine called a finite automaton. This structure is widely used in linguistics for analysing the structure of sentences and is a standard tool in computer sciences; sometimes it is used as a metaphor to describe the way that the human brain functions. In the context of sequential games, a player, instead of choosing a strategy, chooses a machine which implements his strategy. A machine includes four elements:

1. A set of abstract elements, each of which is a 'state of mind'.
2. An indicator of one of the states to be the 'initial state' from which the machine starts to operate.
3. An output function which assigns the action taken by the machine at each state.
4. A transition function which determines what state the machine is moving to after it receives an input, which is a piece of information about other players' actions.

Thus, an automaton is a device which organizes a player's behaviour in the game. It helps us to formulate a verbal statement of the strategy by attaching a name to each of the states. For example, let us return to the seller–buyers model discussed in the previous section. In that model the equilibrium strategies can be described as a $B + 1$ state machine. The set of states includes one state, Right(i), for each buyer i, and an extra state COMP. The state Right(i) has the interpretation of 'i has the right to purchase the good'. The state COMP is the 'competitive state'. The content of a state is determined by its output and by the rule of transition at that state. The initial state is Right(i^*). At Right(i) the seller offers p^* and accepts p^* from buyer i only; when meeting other buyers he offers a non-serious price (above the competitive price) and rejects all offers which come from anyone other than *i*. COMP is a terminal state, in the sense that once the machine gets to COMP it never leaves that state. In this state the seller demands the price 1 and accepts no less than 1. The *B* buyers' strategies are similar in their structure, having the same transition rules. (See Ben-Porath and Peleg, 1987, for presentation of results related to repeated games in the language of automata. See Osborne and Rubinstein, 1990, for presentation of results from sequential bargaining theory using this language.)

Though the automaton is used to facilitate a clear interpretation of the sequential game strategies, its main importance to economic theory is as a convenient analytical tool for introducing considerations of complexity into our models. When players choose a strategy they take into account not only their game's pay-off but also the complexity of the strategy. To do this we first need to formalize the notion of complexity of a strategy; the language of automata is a convenient vehicle for accomplishing this. First steps in this direction were taken in Rubinstein (1986) and in Abreu and Rubinstein (1988). (See also Rubinstein, 1987.) Players were required to find the trade-off between two objectives: reducing the complexity of their strategies as measured by the number of states in their machine and maximizing the repeated game pay-off. Adding the complexity consideration led to a dramatic change in the folk theorem results.

6. Concluding remarks

Let me conclude with the following remarks.

The goal of economic theory

The issue of interpreting economic theory is, in my opinion, the most serious problem facing economic theorists at the moment. Economic theory has gained a prominent place in the study of economics and achieved a remarkable influence on other social sciences as well. None the less, I find many of my colleagues somewhat apologetic about the goals and achievements of economic theory and even speak of a crisis situation. Their concern can be summarized as follows: economic theory is the part of economics which attempts to deal with the real world. It is not a branch of abstract mathematics even though it utilizes mathematical tools. Since it is about the real world we expect the theory to prove useful in achieving practical goals. But economic theory does not deliver the goods. It cannot make predictions anything like those offered by the natural sciences, and the link between economic theory and practical problems, such as how to fight inflation, is tenuous at best. Economic theory lacks a consensus as to its purpose and interpretation. Again and again, we find ourselves asking the question, 'Where is it leading?' (See Aumann, 1987; Binmore, 1983.) My belief as outlined in this chapter is that, notwithstanding its remoteness from the real world, economic theory is about the language which is used in our reasoning about social interactions.

The choice of the model's language

In economics, as in any other field of science, we look for regularities: regularity, however, depends on the language we use to describe a particular situation. Behaviour may be regular or irregular, depending on the language used. If you put blue, red and green objects in front of a subject you may find irregularity in the sense that the subject picks a different colour each time: however, his behaviour may be quite regular if we describe his problem as a choice from among left, centre and right if he always chooses left. If our vocabulary did not include the words for position, we would not be able to describe this regularity. Thus, the correct language for explaining regularities must coincide with the way in which participants perceive a situation and not the way in which analysts perceive it.

Future goals

There are two major fields of research in which language and economic theory can interact. First, tools from economic theory can be used to explain the classification systems individuals use. The function of these systems is connected to human interaction and is therefore likely to be closely linked with equilibrium analysis. Second, language is part of the reasoning process used by decision-makers. Economic models with these processes embedded in them are usually classified under the title 'bounded rationality', a field which has recently attracted wide attention. Is a new economic theory emerging which will abandon the full rationality assumption and instead focus on decision-makers' individualistic procedures? In my opinion the answer is yes, but it will remain for future research in economic theory to determine the answer.

References

Abreu, D., Pearce, D. and Staccheti, E. (1986) Optimal cartel equilibria with imperfect monitoring. *Journal of Economic Theory*, **39**, 251–69.

Abreu, D. and Rubinstein, A. (1988) The structure of Nash equilibrium in repeated games with finite automata. *Econometrica*, **56**, 1259–82.

Aumann, R. (1987) What is game theory trying to accomplish? In K. J. Arrow and S. Honkapohja (eds), *Frontiers of Economics*. Oxford: Blackwell.

Ben Porath, E. and Peleg, B. (1987) On the Folk theorem and finite automata. Research Paper No. 77, The Hebrew University of Jerusalem.

Binmore, K. (1983) Aims and scope of game theory. Mimeo, LSE.

Binmore, K. (1987b) Nash bargaining theory II. In K. Binmore and P. Dasgupta (eds), *The Economics of Bargaining*. Oxford: Blackwell, 61–76.

Binmore, K., Rubinstein, A. and Wolinsky, A. (1986) The Nash bargaining solution in economic modelling. *The Rand Journal of Economics*, **17**, 176–88.

Machina, M. (1987) Choice under uncertainty: problems solved and unsolved. *Journal of Economic Perspectives*, **1**, 121–54.

Nash, J. (1950) The bargaining problem. *Econometrica*, **28**, 155–62.

Osborne, M. and Rubinstein, A. (1990) *Bargaining and Markets*. London: Academic Press.

Pearce, D. (1991) *Repeated games: cooperation and rationality*. Cowles Foundation.

Radner, R. (1981) Monitoring cooperative agreements in a repeated principal agent relationship. *Econometrica*, **49**, 1127–48.

Robinson, A. (1963) *Introduction to Model Theory and to the Meta Mathematics of Algebra*. Amsterdam: North-Holland.

Rubinstein, A. (1977) Equilibrium in supergames. Research Memorandum No. 25, Center for Research in Mathematical Economics and Game Theory, The Hebrew University of Jerusalem.

Rubinstein, A. (1978) Definable preferences relations three examples. Research Memorandum No. 31, Center for Research in Mathematical Economics and Game Theory, The Hebrew University of Jerusalem.

Rubinstein, A. (1979a) An optimal policy for offenses that may have been committed by accident. In S. Brams, A. Schotter and G. Schwodiauer (eds), *Applied Game Theory*. Wurzburg: Physica-Verlag, 406–13.

Rubinstein, A. (1979b) Equilibrium in supergames with the overtaking criterion. *Journal of Economic Theory*, **21**, 1–9.

Rubinstein, A. (1982) Perfect equilibrium in a bargaining model. *Econometrica*, **50**, 97–110.

Rubinstein, A. (1984) The single profile analogues to multi profile theorems: mathematical logic's approach. *International Economic Review*, **25**, 719–30.

Rubinstein, A. (1986) Finite automata play the repeated prisoner's dilemma. *Journal of Economic Theory*, **39**, 83–96.

Rubinstein, A. (1987) The complexity of strategies and the resolution of conflict: an introduction. In B. Bryant and R. Portes (eds), *Global Macroeconomics: policy conflict and cooperation*. London: Macmillan Press, 17–32.

Rubinstein, A. (1991) Comments on the interpretation of game theory. *Econometrica*, **59**, 909–24.

Rubinstein, A., Safra, Z. and Thomson, W. (1990) On the interpretation of the Nash bargaining solution. Mimeo, Tel Aviv University.

Rubinstein, A. and Wolinsky, A. (1990) Decentralized trading, strategic behavior and the Walrasian outcome. *Review of Economic Studies*, **57**, 63–78.

Sen, A. (1977) On weights and measures: informational constraints in social welfare analysis. *Econometrica*, **45**, 1539–72.

Zeuthen, F. (1930) *Problems of Monopoly and Economic Warfare*. London: Routledge and Kegan Paul.

4

Roberto Scazzieri

Actions, processes and economic theory

1. Introduction

Economic research in the twentieth century has made remarkable contributions to the understanding of the economic structure of industrial or industrializing economies. It has also contributed a large and sophisticated body of literature concerned with the pure theory of economic action. Less satisfactory is perhaps the economists' achievement in analysing the relationship between structures and actions, that is, the linkage between economic decisions and the relatively persistent patterns of economic action that can be detected over historical time.

In this chapter, I shall suggest that there is a definite relationship between the present 'scope and status' of economic theory and the historical path that economic science has followed so far, a path that has placed it in an intermediate position between moral philosophy and natural science (section 2). I shall then discuss in which way the above situation may be related to the contribution of a number of classical writers (particularly Smith) in shaping the methodological status of political economy (section 3). In this section, it will also be argued that the gulf between 'natural science' and rational reconstruction in the field of economic studies requires the reconsideration of our Smithian heritage.

The subsequent section (section 4) will suggest that the redirection of economic theory towards closer integration of the analysis of human decisions with that of relatively persistent structures would probably entail the reformulation of a substantial part of economics in terms of a theory of historical processes.

Section 5 gives a brief sketch of the way I see my research work in terms of the above interpretation of the present 'scope and status' of economics.

Research support from the Italian Ministry of University and Science (Project 'Value and Wealth in Classical and Modern Economics') is gratefully acknowledged.

This chapter is dedicated to my son Luigi in the first months of his life.

Finally, section 6 attempts a reconstruction of my academic formation and intellectual experience.

2. Economics between moral philosophy and 'political arithmetic'

It has recently been argued that economic theory lends itself to the formulation of two distinct analytical frameworks, one describing the network of interrelationships among consistent (that is, mutually compatible) beliefs and decisions, the other describing the network of interrelationships among consistent actions affecting the physical world (see Hicks, 1976; Pasinetti, 1981, 1986; Baranzini and Scazzieri, 1986a; Quadrio-Curzio and Scazzieri, 1986). In the former case, economic theory may be considered as a type of rational reconstruction akin to moral philosophy, in the sense that, after assuming a particular objective (the economic motive) as the criterion for human action, the whole network of interpersonal relationships is reconstructed on that basis. As a result, even if it is acknowledged that human actions, in reality, depend upon a number of different (and sometimes conflicting) criteria, it is still found that a proper subject for economic theory is the reconstruction of interpersonal relationships on the basis of a single-minded goal, such as utility maximization or cost minimization.

The other approach considers the motives for action to be historically or institutionally given, and identifies the network of material relationships among commodities, agents and services as a relatively persistent structure that may be analysed on its own grounds. In this case, economic theory is akin to the theoretical frameworks of empirical science, in the sense that the object of investigation presupposes a particular description of the physical world, but is independent of the intensionality of economic agents. In other words, the object of investigation is represented by a network of relationships that can be identified in an objective sense (they are flows of produced and traded commodities, or of services delivered by particular productive funds). The behavioural assumptions, in this case, are important in so far as the dynamics of the system are concerned, but do not affect the identity of the economic system as such.

The latter approach has its roots in the early attempts to apply inductive methodology to the collection and organization of economic data at the level of the economic system as a whole (see the works belonging to the field of *political arithmetic*, such as those of John Graunt (1662), William Petty (1690, 1691), Gregory King (1936, MS 1696) and Charles Davenant (1698).

This approach was also followed by economic writers interested in the process of wealth formation and distribution at the social (rather than individual) level, such as the French physiocrats, Italian economists such as Cesare Beccaria and Pietro Verri, and the British classical economists.

A remarkable implication of the above duality between the 'moral philosophy' approach and the 'political arithmetic' approach to economic analysis is that the most elementary theoretical frameworks corresponding to either point of view look significantly different from each other. The 'moral philosophy' approach has led economists to formulate sophisticated models of rational decision-making and interactive behaviour (see, for example, Edgeworth, 1881; Allais, 1943; Hahn, 1978; Aumann, 1987). In this field, emphasis is placed upon the network of mutually consistent (or mutually interacting) beliefs and decisions, rather than upon the 'objective' system of events brought about by such beliefs and decisions. On the other hand, the 'political arithmetic' approach has led economists to consider any given economic system as an objective structure that is relatively persistent and independent of the behaviour of particular economic agents. In this case, emphasis is placed upon the more stable types of interpersonal relationships, and division of labour emerges as the foundation of economic structure. As a result, the most sophisticated formulations of this approach have considered the economic system as a network of interrelated processes of production (see, for example, von Neumann, 1937; Leontief, 1941; Sraffa, 1960; Pasinetti, 1981). In the following section, we shall argue that the distinction between the above two strands of economic theory may be related to the descriptive duality of economic actions, so that the same economic action may be considered either in terms of the agent's intentions, or in terms of a particular system of events. It will also be argued that 'subjectivist' and 'objectivist' analytical frameworks should be considered as complementary to one another, and that their relationship is best elucidated by considering David Hume's and Adam Smith's theory of commercial society.

3. Disentangling the Smithian heritage: labour funds, capabilities and learning

A distinctive feature of Scottish Enlightenment is the attempt to outline a theory of commercial society based upon the rational reconstruction of agents' beliefs and a detailed empirical analysis of the 'material network' of tasks and interdependencies that characterize the workings of that type of society.

It is a characteristic feature of the Scottish Enlightenment that the identification of the necessary conditions for the consistency of beliefs and the undertaking of advantageous trade is not carried out as an abstract exercise, but is associated with the analysis of a particular type of society, in which 'commerce [i]ncreases industry, by conveying it readily from one member of the state to another, and allowing none of it to perish, or become useless' (Hume, 1987, p. 301; 1st edn 1753). In other words, the society of exchange is identified with a particular set of circumstances such that, as James Steuart pointed out, 'trade and industry [...] are reciprocally aiding and assisting to each other' (Steuart, 1966, p. 146; 1st edn 1767).

In particular, trade is defined by Steuart as a special type of exchange, such that 'the wealth, or work, either of individuals, or of societies, may, by a set of men called merchants, be exchanged, for an equivalent, proper for supplying every want, without any interruption to industry, or any check upon consumption' (Steuart, 1966, p. 146; 1st edn 1767). On the other hand, industry is defined as 'the application to ingenious labour in a free man, in order to procure, by means of trade, an equivalent fit for the supplying every want' (Steuart, 1966, p. 146; 1st edn 1767). The above definitions suggest a type of society in which exchange activities take place among 'free' individuals who are engaged in production activities, so that the pattern of interpersonal relationships reflects the distribution of *capabilities* among individuals (and their evolution over time), rather than the distribution of resource endowments and preferences among such individuals. This approach suggests a view of the economic system as a network of productive funds performing their operations either in isolation or following a certain pattern of task specialization and division of labour.

In other words, the basis of society is identified with the organizational structure that connects specialized producers to one another, rather than with the relationships among individuals who are exchanging existing goods by taking advantage of differences in their respective preference orderings. As a result, society is considered as a network among productive funds (the labourers), who originally have a wide range of capabilities, some of which may be fully utilized, while others may be utilized only partially or not at all. In this context, trade is considered as an organizational structure that allows for a more effective utilization of existing capabilities, by permitting producers to specialize in the actual practice of a limited range of capabilities.

As Adam Smith noted, the narrowing of the range of a worker's capabilities that are actually utilized by him or her has a number of positive effects upon the provision of goods associated with a given labour time. For the practice of a single capability may be associated with shorter periods of idleness, greater accuracy of execution and greater propensity to invent new methods of production.

To the above advantages of division of labour, which are explicitly discussed in Smith's *Wealth of Nations*, one may add the advantage discussed by Melchiorre Gioja (1815–17) and Charles Babbage (1832), according to which division of labour allows producers to avoid the misallocation of capabilities, so that tasks requiring a sophisticated skill will be performed by workers other than those executing tasks of a simple kind.

Exchange is possible without production; but in this case there may only be an efficiency improvement of a static type: the existing provision of goods is simply redistributed according to individual preferences, so that a social welfare function may be maximized. On the other hand, production without exchange is likely to maintain the provision of goods considerably below the level that may be achieved when capability specialization through trade is introduced.

The commercial society considered by Smith is a particular type of economic organization in which a production network is formed by means of capability specialization among 'free' individuals, that is, individuals free to adjust the pattern of utilization of their own capabilities depending on the type of interaction with other individuals of the same network. As a result, the advantage of commercial society is to be found not so much in the *static* maximization of welfare for any given provision of goods, but rather in the *dynamic* maximization of the provision of goods by means of productive specialization and learning.

Smith's theory of commercial society may thus be reconsidered in terms of an analytical model describing a pure labour economy (see also Pasinetti, 1986). This is an economic system in which all commodities are produced by means of labour only, and in which workers (or workers' groups) interact with one another by establishing patterns of 'complex co-operation'.[1] In this case, any given worker (or workers' group) makes a selective utilization of his/her (its) own bundle of capabilities so that the production of any given commodity results from the fabrication of different fractions of the same product by specialist workers or workers' groups. Within this framework, trade may be considered as an organizational device that allows the separate practice of workers' capabilities, so that each worker (workers' group) may restrict his/her (its) activity to tasks requiring a specific skill (or a narrow range of skills). In this way, it is possible to avoid the misallocation of skilled workers to the execution of easy tasks, and the capabilities of the different types can be continuously and fully utilized, provided trade ensures an adequate level of operation for the specialized capabilities of the different types.

Some interesting features of a pure labour economy based upon specialization and trade may be illustrated by considering a simple network representing the relationships among labour funds and productive capabilities. (As will be shown below, the fund-capability network of a pure labour economy may be considered as a first step in approaching more realistic networks in which labour funds are operating side by side with machines and non-produced resources.)

A useful starting-point is the matrix of fund capability relationships (see matrix C below). Any element c_{ij} of this matrix denotes that fund i has a capability that is being used in the provision of intermediate inputs to fund j:

$$C = \begin{bmatrix} c_{11} & c_{12} & \cdots & c_{1k} \\ c_{21} & c_{22} & \cdots & c_{2k} \\ \vdots & \vdots & & \vdots \\ c_{n1} & c_{n2} & \cdots & c_{nk} \end{bmatrix}$$

In the case of complete interdependence, any productive fund i will alternatively appear both on the delivery side and on the 'receiving' side. (For example, it may happen that machine tool x performs a capability essential to the operation of machine tool y, and vice versa.)

A realistic representation of a modern economic system will shown a fund–capability network with many 'loops' of the above type. However, the situation may look considerably different if we consider *broad categories* of productive funds. For example, the 'capability relationship' between any two funds may be of the symmetrical type if we consider these two funds from a purely technological point of view. (For example, a particular worker may have a capability essential to the operation of a lathe, and vice versa.) However, the consideration of broad productive factors (say, labour and machine tools) may suggest a different perspective: the practice of labour capabilities *as a whole* is not necessarily complementary to that of machine-tool capabilities as a whole. As a matter of fact, labour capabilities *in general* may be identified independently of the capabilities of machine tools. (The capability to produce a primitive machine tool is itself independent of the capability of any machine tool in particular.)

The above feature may be represented in our C matrix by assuming that we may identify a block of c_{ij} elements relating different labour capabilities to one another, whereas no other productive fund has capabilities complementary to those of the labour funds. In this case, we may rearrange the rows of matrix C, so that the first g rows refer to types of labour of different capabilities. If we assume that, for the first g rows, whenever c_{ij} holds, c_{ji} also holds, the fund–capability matrix has the following structure:

$$\begin{bmatrix} C_{gg} & C_{gk} \\ 0 & C_{kk} \end{bmatrix}$$

where C_{gg} and C_{kk} are square submatrices, 0 is a rectangular null submatrix and C_{gk} is a rectangular submatrix.

In the above case, the fund capability network is no longer of the circular type, and there is a one-way relationship between the first g productive funds (the labour funds) and all remaining elements. In particular, no fund element belonging to rows $g+1, \ldots, n$ provides a capability that is essential to the practice of the capabilities of the first g elements. On the other hand, fund elements $1, \ldots, g$ provide essential capabilities to fund elements $g+1, \ldots, n$.

So far we have considered the structure of the fund-capability network that emerges from a simple rearrangement of the rows of the C matrix, without asking whether there is a possible relationship between the complete network of fund–capability relationships and the structure of the partial network described in submatrix C_{gg}. As a matter of fact, production systems in which labour and capital fund elements interact with each other to a considerable degree, generally also show a rather complex structure of submatrix C_{gg}. In other words, the co-operation between labour funds and capital funds is generally based upon an extended pattern of 'complex co-operation' (division of labour) among different workers or workers' groups. The above feature of the production network may be shown by a C matrix in which the dimension of submatrix C_{gg} increases over time, and tendentially approximates the dimension of the whole capability space.

The starting-point of the above process may be a situation in which distinct bunches of capabilities relative to labour skills may be clearly identified. In this case, the structure of submatrix C_w (the capability submatrix relative to labour skills) will be as follows:

$$C_w = \begin{bmatrix} C_{11} & 0 & \dots & 0 \\ 0 & C_{22} & & 0 \\ \vdots & & \ddots & \vdots \\ 0 & 0 & & C_{gg} \end{bmatrix}$$

In the above case, each labour skill entails a capability whose practice is independent of any other labour skill. This is a situation that may be found in primitive patterns of complex co-operation, in which each particular labour capability is associated with a specific craft, so that the whole space of labour capabilities could easily lend itself to decomposition in terms of a limited number of independent subsets. The further advances of complex co-operation may be considered as increasing the degree of interdependence among the different labour capabilities, so that the actual structure of submatrix C_w will eventually resemble rather closely that of submatrix C_{gg}. (For example, the capabilities of weavers and loom makers, which are originally independent of each other, will eventually become closely intertwined, in the sense that the weaving technique comes to reflect the technique by which looms are made, and vice versa.)

We may conjecture that the above process is associated with the tendency towards utilization of an increasing number of produced productive funds (instrumental goods). For example, the emergence of complementarity between the weaving capability (a particular type of labour capability) and the 'mechanical' capability of looms is reflected in a complementarity between two different labour capabilities: the capability of weavers and that of loom makers. As a result, the increasing degree of interdependence among *different* types of productive funds may be described in a simplified way by considering the microcosm of the 'labour network' represented by the first g rows and columns of the C matrix.

The analytical device of a pure labour economy may be a first step towards the understanding of the full complexities of a modern industrial economy. This is because the transition from a traditional to an industrial economy is generally associated with an increase in the number of distinct capabilities, due to the increased utilization of advanced instruments. (Such a phenomenon will be reflected by an increased dimension of matrix C.) At the same time, the structure of submatrix C_w will also change, owing to the greater interdependence between tool-using capabilities and tool-making capabilities (see above).

We may conjecture that the above transformation in the structural relationships among labour capabilities is a result of the greater accuracy and standardization of instruments: the more specific *and* specialized a given task becomes, the closer must be the relationship between the labour capabilities of the workers who are making the corresponding instrument and those of the workers who are using such an instrument.

If the above framework is adopted, trade may be considered as an organizational device by which 'distant' labour capabilities may be put into relation to one another, so that the misallocation of capabilities to tasks can be avoided and 'advanced' capabilities can be assigned to specialized labour funds. (In this case, specialized funds are economically feasible because they perform the same type of task in different processes, thus entering the production of a variety of goods.)

One interesting implication of the historical process described above is that there will also be a change in the character of the relationship between labour and produced productive funds.

Any given economic system is 'supported' by a particular network of agents and materials. Workers and machines perform productive operations upon produced or non-produced materials, thus determining in an essential way the characteristics of production processes.

However, the 'active' and 'passive' roles that are associated with agents and materials respectively may vary, depending on the particular production process that is being considered. For example, a given tool could be an active instrument in one process and a 'passive' material in another. (The latter could be a process in which that tool is transformed into a more sophisticated instrument of production.) (See also Scazzieri, 1991a.)

The relationship between agents (usually productive funds) and materials may be represented by a matrix $R \equiv [r_{ij}]$. Any r_{ij} relationship denotes that element i is an agent with respect to element j.

If there are asymmetric fund–material relationships in the economic system, then it is possible to rearrange the rows and columns of matrix R in order to identify distinct blocks of fund-material relationships. In this case, the structure of matrix R will be of the following type:

$$R = \begin{bmatrix} R_{gg} & 0 & R_{gk} \\ 0 & R_{hh} & R_{hk} \\ 0 & 0 & R_{kk} \end{bmatrix}$$

Submatrix R_{gg} contains only symmetric relationships between productive elements of type G (say, different categories of workers), and similarly submatrices R_{hh} and R_{kk} contain symmetric relationships between productive elements of types H and K (say, machine tools and artificial genes respectively).

On the other hand, submatrices R_{gk} and R_{hk} refer to asymmetric relationships between one active element of categories G and H respectively and one passive element (a 'material') of a different type. We may say that, with the above technology, elements of types G and H are 'original' productive funds, in the sense that such elements can be on the 'passive' side of a fund–material relationship only if another element of *their own* group is on the 'active' side. In all other cases, elements of groups G and H are on the 'active' side, that is, they are never acted upon from external agents.

The historical process by which the capabilities of tool makers and those of tool users become closely intertwined (see above) has a counterpart in the field of fund-material relationships. As a matter of fact, the above process suggests that there may also be a significant extension of symmetric relationships between different types of labour funds (that is, of relationships within the G submatrix), quite independent of the utilization of labour as an agent with respect to produced and non-produced materials (this latter type of relationships would be included in submatrix R_{gk}). In particular, it may happen that certain symmetric relationships between machine tools are substituted by symmetric relationships between labour skills. For example, in traditional industrial technology a lathe-making tool may alternatively be an agent or a material, depending on the stage of production that is being considered. In this case, a circular, perfectly synchronized economic system will have machine tools of a given type both as rows and as columns of matrix R. The above outcome depends on the assumption that the capabilities of lathe-making tools are fixed between subsequent time periods. However, modern manufacturing technology often requires the capabilities of machine tools to be changed, and such a requirement may be achieved by means of interaction between machine-tool makers and machine-tool users. As a result, the identity of machine tools on the 'active' side may be different from that of machine tools on the 'passive' side, and the circular relationships between machine tools forming submatrix R_{hh} may disappear altogether. On the other hand, the interaction between makers and users of machine tools could lead to a symmetric relationship included in submatrix R_{gg}.

We may conclude that recent advances in manufacturing techniques (such as flexible manufacturing and 'just in time' production) may be represented analytically in terms of changes affecting both the capability structure of an economic system (matrix C) and its fund material structure (matrix R) (see also Landesmann and Scazzieri, 1991a, b). In the former case, submatrix C_{ll} (relative to labour skills) will show an increasing degree of connectedness, reflecting the process by which previously separate 'crafts' come to form a single pool of interdependent skills. In the latter case, the relative position of different 'original' funds (represented by square submatrices, R_{gg}, R_{hh}, etc.) may be significantly changed, with an increasing weight of labour, which might turn out to be the single truly 'original' fund of capabilities in the whole economic system.

Both historical developments come to reinforce Smith's contention that '[t]he annual labour of every nation is the fund which originally supplies it with all the necessaries and conveniences of life which it annually consumes' (Smith, 1976, p. 10; 1st edn 1776). This also suggests one important reason for the consideration of a 'pure labour' economy as a useful heuristic device if we want to represent analytically some essential features of an advanced industrial economy.

4. A theory of economic history

A distinctive feature of Smith's contribution to economic analysis (but also of Steuart's and Hume's) is the emphasis upon the relationship between the creative behaviour of human beings as *producers* and the acquisitive behaviour of human beings as *traders*.

In the Scottish tradition, production is made more effective by trade, which permits specialization according to skill and encourages the formation of producers' networks (these are networks of producers specializing in the making of partial products). On the other hand, trade among producers permits the expansion in the overall supply of goods rather than the reallocation of existing endowments considered in a pure exchange model.

The analytical core of Scottish theory (and of Smith's theory in particular) is the consideration of a pure producers' economy, in which resource endowments do not significantly constrain the relative economic position of individuals or countries. In addition, production processes are represented in a simplified way by considering the network of tasks forming partial and complete products as well as the patterns according to which tasks are assigned to workers of different skills. As an outcome of the above simplification, the Scottish theory of 'commercial society' may provide a comprehensive treatment of the task–process structure of an economic system, but only a partial and specialized view of the other two fundamental features of a production economy, that is, the agents–materials network and the stock–flow network.

In particular, Smith implicitly assumes that labour capabilities are the most critical ones, since labour skills may be complementary with the capabilities of tools and machines, whereas the interdependence between different types of tools or machines is not considered. The capability matrix, in Smith's case, will thus be a rectangular one:

$$[\hat{C}_{gg} \quad C_{gk}]$$

However, submatrix $\hat{C}_{gg}$ will be of the diagonal type, to show that, according to Smith, the *practice* of each labour skill is independent of any other labour skill (this situation is also represented in matrix C_w above).

In spite of this special assumption, Smith's approach appears to be of considerable generality. For example, the introduction of sophisticated machine tools may be represented by a C_{gg} submatrix with positive elements outside the main diagonal (see above).

The agents materials network of a Smithian production economy may also be represented by a rectangular matrix, as shown below:

$$[R_{gg} \quad R_{gk}]$$

The above case shows that labour is the only 'original' fund of the economic system, since only labour appears both on the 'active' and on the 'passive' side of the fund–material relationships in submatrix R_{gg}.

Finally, the stock–flow network underlying Smith's theory could be represented by the following linear and homogeneous system of equations (see Pasinetti, 1986, p. 422):

$$\begin{bmatrix} 1 & & & & -c_1 \\ & 1 & & & -c_2 \\ & & & & \vdots \\ & & & \ddots & -c_m \\ -l_1 & -l_2 & \dots & -l_m & 1 \end{bmatrix} \begin{bmatrix} X_1 \\ X_2 \\ \vdots \\ X_m \\ L \end{bmatrix} = \begin{bmatrix} 0 \\ 0 \\ \vdots \\ 0 \end{bmatrix}$$

In the above system, production technology is described by the vector of labour coefficients l_i $(i=1,\dots,m)$, per capita demand by the vector of consumption coefficients c_i $(i=1,\dots,m)$, and the total outputs of the m different commodities by the vector of physical quantities X_i $(i=1,\dots,m)$. The above representation of input–output flows describes a pure labour economy, that is, an economic system in which '[a]ll consumption goods are made by labour alone; labour is therefore the only "factor of production"' (Pasinetti, 1986, p. 421).

This type of economic system may appear to be quite distant from a modern industrial economy, in which division of labour is associated with the formation of a 'circular' commodity network such that, for instance, steel is an input in the production of chemicals, and chemicals an input in the production of steel. However, a closer look at some recent transformations of production technology may suggest the contemporary relevance of certain features of a pure labour economy. In particular, the introduction of flexible production techniques implies that it may be difficult to identify in an unambiguous way the interrelationships among commodity flows that characterize any given circular network. (Strictly speaking, a circular network of intermediate products may be identified provided we have a fixed technology in use; in this case only, the economic system reproduces, or expands, itself by maintaining its fundamental structure unchanged. The substitution of one technology for another makes the empirical analysis of the circular flow more difficult, since the output vector of the *old* technology is used to support the introduction of a *different* set of technical coefficients.)

Flexible methods of production make the circular flow not only difficult to detect from an empirical point of view, but also difficult to investigate from a theoretical point of view. As a matter of fact, flexible methods are based upon a relationship between processes rather than between commodity flows; for example, a process delivering enzymes could be related to one delivering a family of chemicals quite independently of the particular outputs of the two processes at any given time.

This feature of flexibility has one important implication: the relationship among 'objective' commodity flows recedes into the background, while the relationship among the capabilities embedded in existing processes comes to the fore. As a matter of fact, the mutual consistency among different processes is often achieved by means of adjustment and learning involving a type of interaction between different working skills.

As a result, the analysis of commodity flows suggests in this case the point of view of 'vertical integration', according to which

> all intermediate circular processes fulfil an ancillary role. The economic system appears simply as being made up of as many vertically hyper-integrated sectors as there are consumption goods. Each vertically hyper-integrated sector *i*, when operating at unit activity, may be represented by the elementary vector: $[1, 1, l_i, (g+r+i)]$, $i = 1,2,\ldots, m$, where the first component refers to one unit of consumption good *i*, the second component to one unit of vertically hyper-integrated productive capacity for consumption good *i*, the third to the physical quantity of (vertically hyper-integrated) labour for consumption good *i* and the fourth to the growth rate of demand for consumption good *i*. (Pasinetti, 1988, pp. 133–4)[2]

In other words, the economic system appears now as a set of funds characterized by a varying degree of reproducibility and delivering productive services in a flexible way. In particular, an economic system using capital and labour funds to produce consumption goods may ultimately be reduced to a pure labour economy, owing to the fact that the capital goods, provided they are considered as productive funds rather than as elements of the circular flow, may be reduced, in a sequence of steps, to a sum of weighted quantities of labour plus a physical residual that becomes infinitesimally small if we perform the reduction a sufficient number of times (this property is discussed in Pasinetti, 1973, pp. 15–17). In other words, the utilization of capital goods within an economic system is no obstacle to the analytical reduction of such a system to a pure labour economy, provided capital goods are considered, at each step, as 'capital funds' rather than produced commodities. If the above point of view is adopted, capital goods may ultimately be considered as pools of technical capabilities, which generally derive from the capabilities of labour. In this case, the overall 'capital fund' of any given economic system appears as a set of instruments strengthening the productive potential of its labour fund (see, in this connection, Rae, 1834, Book II).

On the other hand, the circular network of intermediate products, which is sometimes necessary for the existence of a capital fund, recedes into the background. As a result, the vertical structure of the production process is emphasized, in the sense that the production of goods and services is considered as the result of labour capabilities, quite independent of the material instruments required for the actual practice of capabilities. At the same time, the interdependence among different types of labour capabilities may be substituted for the interdependence among flows of intermediate commodities. For the productive structure of an advanced economic system is associated with the emergence of a well-identified 'capability network', in which different labour capabilities come to be closely intertwined (see above). It is possible to argue that, in this type of society, such a capability network replaces (as a relatively permanent set of relationships) the more 'material' blocks of capability relationships (see matrix C above) and fund–material relationships (see matrix R above). Of course, intermediate commodities continue to be produced in an interdependent way, but the 'objective structure' of the corresponding input–output flows is no longer a fundamental object of investigation. On the other hand, it is now essential to consider the economic system as a set of productive funds whose range of capabilities is given within relatively long time periods, whereas their mode of operation may vary from one production programme to another. In particular, such a set of funds may ultimately be reduced to labour capabilities and the natural environment. The former determine in which way available machines and resources can be put to use (and also which types of instruments can be made), the latter identifies boundaries to the effectiveness of existing capabilities, and often determines the focus of the learning process by which labour capabilities are transformed in the course of time.

The above argument suggests a new perspective in the evaluation of the relative position of exchange and production within the framework of an advanced production economy.

From a general point of view, we may identify at least three different types of relationship between exchange and production. First, exchange may be independent of any significant relationship with the technical skills of producers. This may be the case if traded goods are non-producible, or even if they are produced but not exchanged on a regular basis. Here, the exchange relationship reflects the resource endowments and preferences of economic agents and is virtually independent of available technology.[3] Second, exchange and production come to be closely intertwined, so that the practice of exchange, by becoming customary, influences the characteristics of productive activity. (For example, the need to satisfy a large demand for textiles may induce a change from handicraft production to industrial methods.) On the other hand, the standardization of production techniques, once it becomes widespread, tends to bring about a close correspondence between terms of trade and conditions of production. The most advanced stage of this process is the situation in which price setting becomes a function of business accounting (mark-up pricing), so that the linkage between the availability of a commodity and the demand for it (what may be called the 'scarcity relationship') becomes relatively unimportant in price determination. This type of set-up is one in which, as Sir John Hicks has argued, price setting is no longer carried out by an independent body of people (the 'merchants'), but is undertaken by producers themselves, particularly in the case of what has been called 'intra-industry trade' (that is, trade between producers of specialized parts within a given industry). In the above situation, '[t]he merchant must [...] be acting [...] as the manufacturer's agent. So we have here an important example of the *vertical integration* [...]; manufacturing and selling come in substance under the same control' (Hicks, 1989, p. 24).

Finally, a third type of situation may be one in which exchange comes to be closely connected with producers' skills in a more direct way, that is, in a way that reflects the dynamic interdependence among producers' capabilities rather than the given set of material techniques (as is the case with the second type of situation considered above). Here, sets of exchange relationships may emerge that reflect existing productive networks but, on the other hand, the dynamics of productive networks could be the outcome of the evolution of 'power relationships' at the exchange level. In particular, trade relationships may be considered as the outcome of the interplay between material and institutional factors under given historical circumstances. As a result, the existing pattern of exchange relationships (with an 'equalitarian' distribution of economic power, or the dominating position of one or more trading partners) may partly depend on the available set of labour capabilities but, on the other hand, the *actual pattern* of utilization of these capabilities may also reflect the social and institutional constraints upon division of labour. This implies that the existing organization of the exchange network could exert a major influence upon the structure and dynamics of the production network, particularly under conditions of flexible manufacturing techniques. On the other hand, the dynamics of producers' capabilities could become a major influence upon the reshaping of trade relationships in the course of time.

We may argue that recent developments in the field of production technology and organization, by calling attention to the role of labour as an original factor and to the dynamics of labour capabilities, bring to the fore the trade–production relationship associated with the third type of situation considered above. In particular, such developments emphasize the importance of learning processes, and the associated interaction between division of labour and institutional set-up in a dynamic perspective.

5. Towards a structural theory of economic action

The best way to see how an economist relates his or her work to a general evaluation about the research perspectives within his or her field, is perhaps to assess problems and unsolved issues that have emerged within one's own activity. This is the case, not only because to discuss internal tensions within a given research programme may suggest new lines of investigation, but also because to expose the essential features of an unsolved issue is often a prerequisite for the identification of a more comprehensive and satisfactory analysis.[4]

The starting-point of my approach to economic research may perhaps be put in terms of the distinction, and interaction, between an *order of being* and an *order of doing*.[5] The former refers to the existence of an objective structure and is often rooted in the material interdependence among production processes and other nature-related activities. The latter describes connected patterns of transformation associated with the 'free' activity of human beings.

The field of technical choice under conditions of an exogenously given, and exogenously changing, scale of production, seems to provide an interesting perspective upon the above distinction.

As a matter of fact, it is possible to show that the philosophical duality between natural and subjective rankings (see also Urmson, 1950)[6] suggests the *economic* distinction between rankings of two different types. In the former case, one has a ranking of economic states based upon a given physical characteristic independent of the preferences of economic agents; in the latter case, one has a ranking of the same states according to the preference structure of the relevant agent. Any economic ranking presupposes one or more physical rankings, but cannot be reduced to the latter.[7]

The above distinction suggests a particular interpretation of the relationship between scale and productive efficiency, according to which a variation of process scale influences the 'productivity' of the process not directly (as would be the case under the law of variable proportions) but *indirectly* as a result of a scale-induced contraction or expansion in the producer's choice set (see Scazzieri, 1981, 1982, 1992a).

For example, a scale increase could bring about a contraction in the number of available techniques and a producer who is trying to follow the 'best practice' technique for any given scale might be obliged to introduce an inferior technique.

The resulting 'decreasing efficiency' of technology in use is thus the outcome of the combined influence of a physical process (the reduction in the number of available techniques) and of an economic process (the adoption of a particular technique from among the set of the available alternatives).

The above relationship between scale and productive efficiency suggests the possibility of analysing economic phenomena in terms of the distinction between 'actions' and 'events'. The former are intentional patterns of behaviour associated with some transformation in states of the world (see, for instance, von Wright, 1963, chapter II, 'Preliminaries on logic. The logic of change'). The latter are happenings by which a given state of the world is changed (we may note that sometimes change takes place through a sequence of connected events, thus determining a particular *process* of transformation) (see, on the distinction between events and processes, von Wright, 1963, pp. 25–6).

The relationship between scale and productive efficiency suggests the possibility of distinguishing between 'objective efficiency' and 'subjective efficiency'. The former reflects a natural ranking of states of the world independent of economic preference and choice; the latter presupposes an arrangement of states of the world (among which one or more natural rankings have been introduced) so as to reflect the 'subjective' or institutional features that determine actual economic behaviour.

The above distinction may in turn be related to the general possibility of analysing the outcome of economic actions in terms of 'subjective' and 'objective' criteria. In particular, 'objective' features may be detected not only on the level of natural laws but also when considering the different layers of interdependence among economic agents. For example, the interaction among individuals could bring about institutions or social practices that become in turn 'objective' rules influencing individual behaviour on a different level. On the other hand, 'objective laws' within the economic sphere may generally be explained as the outcome of interaction between objective and subjective factors. For example, the interaction between the natural criterion of non-producibility of land and the 'subjective' criterion according to which technical alternatives may be ranked could explain the classical law of decreasing agricultural returns (see Sraffa, 1925; see also Scazzieri, 1981, 1982).

At a closer look, the interplay between objective and subjective features suggests a more 'historical' view of the production process, in which the role of real duration and 'complementarities over time' (Hicks) is emphasized. In other words, the hierarchical structure connecting subjective and objective features (see above) may be associated with different speeds of adjustment of different parts of the economic system, since 'objective' features would normally be slower to adapt to dynamic impulses than features such as human decisions that immediately reflect the intentionality of economic agents.

In particular, the production process may be represented as a network of capabilities, tasks and flows of materials in process (semi-finished commodities) (see Landesmann and Scazzieri, 1991a,b; Scazzieri, 1992a). The consideration of any of the above three dimensions of production activity suggests a precise relationship between history and freedom of choice. For example, if the capability network is analysed, we find that potential capabilities may generally be associated with existing fund inputs (such as workers or machines), and that the actual patterns of utilization of such capabilities may be different depending upon the form of production organization and the product mix. As a result, past history determines in an important way what can be done at any given time, but the precise form of capabilities' utilization also reflects the intentionality of economic agents, that is, the way in which they represent the future states of the world, and the manner in which they relate themselves to them.

Similarly, the consideration of the task network suggests a particular way in which different primitive operations are connected to each other and to the external environment. In principle, the identification of a task network is independent of the agents performing the functions specified within the network, and also of the materials in process upon which the tasks are executed. (In this sense, the task network is an 'abstract network' with respect to the agents and materials involved in productive operations; on the distinction between 'abstract' and 'implemented' networks, see also Marschak and Radner, 1972, p. 268.) At any given time, a set of tasks is performed that must be compatible with the tasks previously performed within the same production programme. On the other hand, current tasks may be related to future tasks in a non-deterministic way, for current activities are often related to a range of future production programmes rather than to a single sequence of operational set-ups.

Finally, materials in process also present an interesting combination of determinism and 'choice'. As a matter of fact, any intermediate stage of fabrication may be considered, particularly within the modern set-up of flexible production, both as the solution of a previous choice problem and as the opening up of some future options. The material in process at time t may often be moulded in a variety of ways, whose number may be greater if the processing of materials has been split into a large number of elementary component materials (see also Landesmann and Scazzieri, 1991b; Scazzieri, 1991a).

To conclude, structural constraints and dynamic learning (learning about a changing environment) seem to be critical features of the duality between 'objective' and 'subjective' representations of economic actions. In this connection a fruitful route to conceptual progress within our discipline may be suggested by the classical relationship between moral philosophy (a general theory of goal-setting) and political economy (a specialized theory of goal-seeking). In Smith's case, the *Theory of Moral Sentiments* (Smith, 1759) outlines the foundations of a general theory of action evaluation, whereas the *Wealth of Nations* discusses the specialized theory of the class of actions motivated by self-interest (see also Bagolini, 1975).

In a similar way, learning and adaptive behaviour may represent the general, subjective side of economic actions. On the other hand, structural constraints and bottlenecks could be considered as the specific form taken by economic interaction within any given historical set-up.

6. Some personal recollections

It is not easy, and is somehow embarrassing, trying to take stock of one's own intellectual experience at the relatively early age of 41. I have persuaded myself that the attempt was a worthy one partly because of the editor's insistence and partly also because there may after all be some objective interest in tracing one's own steps backwards, if this will highlight the complementarities between distinct and apparently distant intellectual traditions.

I feel that a most important encouragement, during my childhood in Bologna, was that from my parents. My father, I think I may confidently say, was always very positively related to my interests and efforts, both in a material sense with the supply of reading material, and in an intellectual sense with his ability to impress upon me the idea that I was doing something interesting and valuable. He was rather well read, particularly in science, and his frequent talk about the history of scientific discoveries left in me the feeling that knowledge is dynamic, and the search for it a lively and enjoyable activity. My mother was no less important, with her unreserved happiness with what I was doing and learning, without ever being pushy or nervous about school achievements. Other members of my family were also important in my intellectual development, particularly one of my aunts and her son, an older cousin of mine. They were both very lively and travelled quite a lot, so that I could absorb and enjoy their stories about other parts of Italy, and the world.

My school education proceeded steadily. At almost no point, I must say, did I feel too oppressed with homework and similar duties, so that I could browse in my father's library, picking up books for late afternoon or summer reading (novels, but even more history and, at a later stage, philosophy as well). I spent my high-school years at one of the two state classical schools of Bologna (the Liceo Minghetti), where I thoroughly enjoyed Greek, Latin and Italian poetry (rather than prose), philosophy and some sciences (biology and chemistry rather than physics). From my high-school time, I feel I really acquired a training in interpretation and construction, which is of course related to classical studies but had considerable spill-overs in the philosophy and biology training. My high-school philosophy was mainly continental, and I remember I particularly enjoyed reading Kant's *Prolegomena* and Schopenhauer's aphorisms (but also, during my final year at the high school, Marcuse's *One-dimensional Man*).

Following an established tradition in Bologna, there was really no question of attending university anywhere else. However, I was lucky enough in finding available a new curriculum at the Faculty of Political Sciences, which allowed me to follow a path really quite close to the Politics, Philosophy and Economics (PPE) Honours School at Oxford. I was able to pursue my historical and philosophical interests for almost two years more, while starting at the same time a systematic study of economics, which took the whole of my time in the third and fourth years.

The intellectual environment at the university was lively, and I remember attending lectures in other faculties as well, such as a course on logic and philosophy of science by Alberto Pasquinelli, who had been with Rudolph Carnap at Chicago, and another on medieval economic ethics by Ovidio Capitani, a distinguished medieval historian. Among teachers of my first university years, I should especially recall Nicola Matteucci, a political theorist with a crisp awareness of the historical dimension of analytical categories; Carlo Poni, a distinguished historian of agricultural and proto-industrial forms of productive organization, and Alberto Quadrio-Curzio, a theoretical economist especially interested in the modern formulations of classical economic theories who had already contributed a major work on rent and distribution in multisectoral models (Quadrio-Curzio, 1967).

I think I must say that Quadrio-Curzio's introductory economics course was a critical turning-point in my university education. There I got the feeling that economic theory could be a field blending history and abstraction in a constructive way, a blend for which I felt I might have an inclination. As a matter of fact, as I was to discover later, Quadrio-Curzio's approach to economic theory, then apparently rather close to that of Piero Sraffa and of post-Keynesians such as Luigi Pasinetti, was itself the product of a sophisticated blend of different intellectual traditions, combining an interest in the multisectoral tradition with a special inclination for free mathematical *and* conceptual construction due to the influence of Carlo Felice Manara, a distinguished mathematician belonging to the Italian school of geometry. Manara had himself been deeply influenced by Federigo Enriques, the mathematician and philosopher of science who had always insisted upon the importance of free conceptual construction in empirical science.

I spent my two final years at Bologna as an internal student within the small Economics Institute that had been organized at the Faculty of Political Sciences by Beniamino Andreatta, a post-Keynesian economist, with the aim of developing an economics curriculum distinct from the business orientation then prevailing at the Faculty of Economics and Commerce. Andreatta had surrounded himself with a set of younger economists who became my teachers, such as Giorgio Basevi, always clear and crisp with his international trade lectures; Carlo D'Adda, a fine connoisseur of Keynes's *General Theory* (which he insisted we should read in the original); Paolo Leon, the brilliant student of structural change and development, and Romano Prodi, the economist with an impressive knowledge of industrial affairs who was to become chairman of IRI but later decided to come back to academic life.

The Economics Institute in the early 1970s was a lively environment, contacts with teachers easy due to the small student numbers, and foreign visitors frequent. I recollect with special pleasure a memorable set of lectures given in 1973 by Michio Morishima when he was working on his book on the mathematical reformulation of Marxian economic theory.

In my final year I attended another course given by Quadrio-Curzio. This time it was an advanced course on multisectoral modelling, capital theory and structural dynamics. It was an impressive course, wide-ranging in scope and yet compact in presentation: a beautiful introduction to some difficult topics in the theory of production and distribution.

At that time, I decided to work with Quadrio-Curzio for my undergraduate dissertation. The chosen topic was the relationship between productivity laws (the 'laws of return') and the theory of value, and the main source of inspiration the 1925 paper by Piero Sraffa, 'Sulle relazioni fra costo e quantità prodotta' (Sraffa, 1925). I remember spending almost one year on the dissertation, going over the writings of the classical economists, of a number of neo-classical writers (mainly Walras, Marshall, Edgeworth, Wicksteed, Pigou), and of Piero Sraffa himself. As a supervisor, Quadrio-Curzio was at the same time supportive and reserved, always careful in not imposing a personal view. He was also very generous with his time: I remember spending a full afternoon (I think it was on a Saturday) going over Sraffa's 'Dr Hayek on money and capital' with him (see Sraffa, 1932).

In January 1975, I matriculated as a graduate student at Oxford, where I had been admitted to Linacre College. In Oxford I was going to spend, although with some interruptions, about five years, working first on an MLitt dissertation on 'Scale and efficiency in classical and post-classical models of production', and later on a DPhil thesis on 'Tasks, processes and technical practices'.

From the beginning, Oxford impressed me with its intellectual 'high life', which made it so different from Bologna. It was clearly an elitist institution, and proudly so, in which membership of the university made one feel more a kind of aristocrat than a fellow artisan within a laborious craft (as it had been at Bologna). The difference had certain advantages: people took casual social interaction much more seriously, and this made me breathe fresh air. It was possible to cross the boundaries between the professional and the amateurish without feeling that one was wasting one's time. In a sense, interaction with people, though quite rigidly predetermined in terms of times and places, was deeper and freer: real 'conversations' (in Michael Oakeshott's sense) were possible, in which time was not an obvious constraint and the same seriousness and detachment could be applied in considering grand themes or tiny matters of everyday life. There certainly was a deep awareness of the social conditions making the academic life style such a rare and worthy experience.

In Oxford I regularly attended lectures and classes, some of which were outstanding (such as Sir John Hicks's on welfare economics and on causality, Amartya Sen's on fundamental economic concepts, economic methodology and social choice, Michael Bacharach's on game theory, J.A.C. Brown's on input–output models, Christopher Bliss's on disequilibrium macro-economics, James Mirrlees's on general equilibrium and growth). I sometimes went to philosophy lectures (such as Dr Newton-Smith's course on scientific revolutions) and attended evening meetings, especially in Holywell Manor, a graduate house belonging to Balliol.

A number of distinguished economists visited the university. I recall in particular a set of guest lectures on Michael Kalecki (one of which was from Joan Robinson), and one evening talk, also from Joan Robinson, on the capital theory debate.

Apart from all this, the really special character of my relationship with Oxford derived from personal acquaintances, which made intellectual communication lively and deeper. Here, I must mention the circumstances that allowed me personally to know Sir John Hicks, who had retired from his Drummond Chair some time previously (in the mid-1960s). Sir John's wife, Lady Ursula Hicks, was a Fellow of Linacre College, to which I belonged as a student. She had been appointed my moral tutor, and this brought me closer to her, and to her husband, when they were both attending the Thursday guest nights and, in particular, the after-dinner conversation in Linacre common room (here, the fact that fellows and students shared the same common room facilities was a special advantage). I am sure that I must have been introduced to Sir John on some early occasion after my arrival in Linacre. However, the way in which I would get to know him more directly is a characteristic instance of Sir John's openness and generosity.

I remember I had been attending one of Sir John's lectures on 'Welfare economics: scope and status' in All Souls College old library, and I was still hanging on at the end of the lecture in order to collect my notes from the table, when Sir John, who had previously announced his availability to students on Thursday afternoons in his office, came to me saying: 'Perhaps you would like to see me this afternoon.' Of course I went, and this was, as far as I recollect, the real beginning of a long personal relationship that lasted until 1989 (the year of his death). In term time, I usually met Sir John in Linacre common room after the Thursday dinners. Normally his wife would have been attending the guest night, and he would come to join her, often coming from All Souls accompanied by Dr Stuvel, the economics statistician. Quite often we would sit together in a small group, and a kind of division of labour would take place between him and his wife: Lady Hicks would run the conversation, asking people around and telling stories about their travels; Sir John would sit in silence, often smoking or chewing his pipe, but every now and then he would get excited about something, and this would often be the kind of excuse for him to tell people about recent readings or his own current writing. It was in this way (I think it was in 1978) that I was able to follow, almost week by week, his work on *Causality in Economics* (Hicks, 1979), from the early chapters on concepts of causality to the late section on probability.

In 1980 I left Oxford to take up a lectureship at Bologna, but we continued to meet every year, often both in England and in Italy. I think it is from him, and perhaps more from his conversation than from his writings, that I could get a feeling of the sophisticated blend of abstraction and concreteness that economic theory can be, particularly if economics is considered as an introduction to the *theory* of economic history (see also Scazzieri, 1992b).

The other senior member of the university with whom I was able to develop a long personal relationship was Michael Bacharach. Our acquaintance was initially more formal, since he had been appointed my university supervisor on the advice of Professor J. A. C. Brown, who had interviewed me on my arrival. I originally had an interest in scale-dependent input–output models, and he had done an outstanding piece of research on the estimation of technical coefficients in input–output models using the 'biproportional method' (Bacharach, 1970). This, I think, might have been one of the reasons for my allocation to him. Our relationship (and also its intellectual content) slowly changed its character in the course of time. I was becoming more historical and he was becoming more philosophical. Partly as a result of this interaction, my own work gradually took a shape that I could not have foreseen on my arrival in Oxford.

I became increasingly involved in the formulation of a theory of human action in production processes that owes a good deal to Michael Bacharach's penetrating criticism of its axiomatic foundation and conceptual development (see Scazzieri, 1992a). In particular, Michael's sophisticated perception of the structure of beliefs and decisions in economic life (see, for instance, Bacharach, 1986, 1988, 1989) made him an ideal critic of certain features of my own approach from the point of view of the philosophy of mind. I think that important features of my work on production theory are related to that criticism, in particular the distinction between the feasibility stage and the deliberation stage of technical adoption. More generally, Michael's presentation of the arguments of economic theory in terms of a complex hierarchy of beliefs (that includes descriptions) was an important stimulus for my own growing interest in the economic 'constitution' of society (see Scazzieri, 1989, 1991b; Baranzini and Scazzieri, 1990a; and the present paper).

My Oxford years also offered me a marvellous opportunity to get to know contemporaries and to develop a network of friendships that was going to be an essential stimulus and support later in life. The first friend that I should recall in this connection is perhaps Michael Landesmann, a graduate from the University of Vienna who was then at Balliol. We first talked to each other in the tower of Nuffield College library and we soon discovered that we had many interests in common: essentially history and philosophy. We also found ourselves sharing the belief that economic theory should be primarily concerned with the historical dynamics of forms of organization, particularly in the production sphere. Already at that time, Michael was a tireless organizer of meetings and 'private seminars', and his room became one of the most intellectually lively places for young economists in Oxford. A few months after meeting him, I came to know Mauro Baranzini, a Swiss Florey student from Queen's. He was interested in post-Keynesian economics, though in his own peculiar 'micro-economic' way. He was slightly older than me and was appointed a lecturer at Queen's immediately after completing his DPhil with James Mirrlees. I remember we started doing some common work together (on growth and distribution). Then he decided to organize a weekly seminar at Queen's, which was attended by graduates and visitors (some of the papers discussed there are published in Baranzini, 1982). Other Oxford friends included Roberto Cippà, also a Swiss graduate from Fribourg, who was later to complete a DPhil on 'consistent temporary equilibria' under the supervision of Christopher Bliss. Roberto and myself used to engage in interminable discussions on economic theory, and his sharp criticism often made me completely rethink the structure (though not always the basis) of rather complex arguments.

On my return to Bologna, I found that the situation there had changed considerably from the early 1970s. A new and larger Economics Department was taking shape, some had left (Quadrio-Curzio had moved to the Catholic University of Milan), others had arrived, such as Salvatore Baldone, a pupil of Pasinetti, Stefano Zamagni, a pupil of Hicks who had been in Oxford before my arrival and who was later to organize in Bologna the International Economic Association Conference 'Value and Capital Fifty Years Later' (see McKenzie and Zamagni, 1991), and Jan Kregel, the post-Keynesian economist, who was then teaching at the Bologna Center of Johns Hopkins University and was later to take up a Chair at the University of Bologna. Altogether, the rather cosy atmosphere of my student years had disappeared, but more resources were available and a constant stream of foreign and Italian visitors was coming in. My lectureship was at the Political Sciences Faculty, where I was required to teach first growth then micro-economic theory. Later on, I taught an additional course on modern formulations of classical theories.

In the early 1980s, I was a commuter between Bologna and Milan, where I used to go almost weekly to see Quadrio-Curzio and to work with him on a series of projects (see Quadrio-Curzio and Scazzieri, 1977 82, 1983, 1986). I became a regular associate at the Catholic University of Milan, where I was able to use the research facilities of CRANEC, a new research centre set up by Quadrio-Curzio and involving Pasinetti, Manara, Baranzini and other scholars. At the Catholic University I had the opportunity to meet personally economists such as Siro Lombardini, always enthusiastic about new theoretical developments supporting his deeply rooted distrust of methodological individualism, and Luigi Pasinetti, who had returned to Milan from Cambridge and was then giving the finishing touches to his *Structural Change and Economic Growth* (Pasinetti, 1981; see also Pasinetti, 1992, for a more recent and essential formulation). My relationship to Pasinetti took some time to develop, but I was eventually able to enjoy his subtlety and learning, which gave me the experience of a style of thought that I had never encountered before, a style combining analytical perceptiveness with an almost exceptional ability to disentangle complicated issues to reveal an apparently simple conceptual framework. Later, we also worked on some common projects, particularly for the *New Palgrave* (see Pasinetti and Scazzieri, 1987a,b). My visits to Milan allowed me to continue my previous acquaintance with Mauro Baranzini, who had been appointed lecturer at the Catholic University.

Around this time, after publishing an Italian version of my Oxford MLitt thesis (Scazzieri, 1981) and another couple of papers connected with it (Scazzieri, 1982, 1983), I became interested in the issue of alternative conceptual frameworks in economic theory. My work with Quadrio-Curzio (see above) attempted a rational reconstruction of the history of economic theory in terms of the interaction between the two conceptual frameworks of scarcity and producibility, an idea originally suggested by David Ricardo (1817; particularly chapter I, 'On value') and recently taken up by Pasinetti (1965, 1981) and Hicks (1975, 1976), which we tried to use in order to work out an interpretation of the dynamics of economic theory in the period between Ricardo and Keynes Immediately after that, I collaborated with Mauro Baranzini on a project for Basil Blackwell, the volume *Foundations of Economics* (Baranzini and Scazzieri, 1986b), which we conceived with the aim of putting together a set of original essays considering the duality between scarcity and producibility from the point of view of modern economic theory. Eventually we felt quite happy with the final outcome, for we were able to persuade an interesting group of established as well as younger economists to send papers,[8] and the volume appeared to be a rather unconventional, even if perhaps controversial, piece of economic methodology (see, for example, the review articles by Cencini for the *Revue d'économie politique* (Cencini, 1986) and Gramm for the *Atlantic Economic Journal* (Gramm, 1987).

During the early 1980s I had never lost touch with Michael Landesmann, but it was really in 1985 that we started working together on some clearly identifiable project. I think the opportunity arose from a month's leave from Bologna in October that year. I spent the whole time in Cambridge (UK), and this gave me the chance to meet Michael a lot, since he was working there as a Fellow of Girton College and a Research Officer at the Department of Applied

Economics. I remember that, during that stay, I had the opportunity to meet Sukhamoy Chakravarty, who was in Cambridge on sabbatical leave from the Delhi School of Economics. I was deeply impressed by his aristocratic type of scholarship, the enormous range of his reading, his ability to overcome narrow specialization. On top of everything, he was a wonderful man, totally unassuming and always willing to listen. He also totally lacked the insularity that may sometimes be found in Oxbridge (and in Italy). In a sense, he was the most open-minded and 'universal' scholar that I have met so far.

In Cambridge, I gradually became involved (although, initially, only as an external observer) in a project on the structural dynamics of economic systems that had arisen from a discussion group including Chakravarty, Richard Goodwin, Michael Landesmann and other economists. Michael asked me to write a paper on the contribution of the classical economists to traverse analysis (Scazzieri, 1990), and we gradually conceived a new, common research project. The idea was really a development of our Oxford discussions ten years earlier, when we had been fascinated by the (then) relatively new concept of Nicholas Georgescu-Roegen about the production process (an issue in which, as we discovered, Chakravarty was also particularly interested) (see Georgescu-Roegen, 1969, 1970, 1971). We thought that structural theory needed 'micro-foundations', and that the type of descriptive modelling started by Georgescu could be a suitable basis in order to build a bridge between the multisectoral tradition and a more detailed consideration of qualitative and historical features (such as technology characteristics, changing consumption patterns, and so on) (see Landesmann and Scazzieri, 1990, 1991a,b).

At this point, my work with Michael Landesmann came to overlap with certain ideas I had elaborated a few years earlier with Quadrio-Curzio. It was the discovery that alternative descriptive frameworks considering the economic system either as a circular flow or as a 'one-way' stream of interconnected activities (horizontal and vertical representations respectively) should really be seen as complementing each other, in the sense that the analysis of historical dynamics may require the concentration of attention associated with one or the other scheme (see Quadrio-Curzio and Scazzieri, 1982a,b, 1986). This point of view is related to some earlier work by Pasinetti on the vertical integration of productive processes (Pasinetti, 1973), but its immediate source was probably Quadrio-Curzio's distinction between 'global technologies' and 'composite technologies',[9] which is the basis of his most recent work (Quadrio-Curzio, 1975, 1986, 1990; Quadrio-Curzio, Manara and Faliva, 1987; see also Quadrio-Curzio and Pellizzari, 1991). As we were to discover later, Sir John Hicks had been thinking along similar lines, particularly in his *Methods of Dynamic Economics* (Hicks, 1985).

Around the mid-1980s I started working with Mauro Baranzini on a project concerning the distinction between horizontal and vertical representations of economic structure. Our idea was to assess that distinction from the point of view of its implications for the analysis of real economic systems. In the end, the project took shape as a co-operative enterprise, in which we were joined by a group of other economists interested in the relationship between theoretical frameworks and economic description (and economic history). The outcome of this work was published by Cambridge University Press (see Baranzini and Scazzieri, 1990b).[10]

My acquaintance with Cambridge continued into the late 1980s. I have been a regular visitor there almost every year since 1987, with a relatively long stretch in the autumn term 1989. During my visits (sometimes with my wife Cristina), I thoroughly enjoyed the magnificent University Library and worked with Michael Landesmann. It is around this time that the different strands I had been pursuing came, so to speak, together, with the start of *Structural Change and Economic Dynamics*, a new journal from Oxford University Press in which Michael Landesmann and myself were joined by a group of economists living in different parts of the world.[11]

During the 1980s a number of important personal events changed my life considerably. In 1983 my father died (it was fortunate that he was able to see me since I was back in Bologna) and I married Cristina, a librarian at Bologna University library. I am considerably indebted to her not only for encouragement and support but also for something quite closely related to her own intellectual habits. For she is a rare-book librarian, and I have often been fascinated by the combination of analytical and historical skills that her profession requires. Following the traces of an *incunabulum*, I find, requires an ability to identify meaningful features, and to relate them within a conceptual construction, which is not so different from what theoretical investigation in economics – or perhaps generally in science – tries to do. Partly as a result of my acquaintance with the world of librarianship, I started to collect, in a rather casual way, early editions of economics classics, with a concentration on late-nineteenth- and early-twentieth-century books, which I could afford more easily. Thus, I do not have any early Ricardo or Mill, but I do have first editions of economists such as Cairnes, Jevons, Wicksteed, Pantaleoni and Pareto.

In 1987 I was appointed Professor of Economics at the University of Padua. There I experienced an exquisite academic environment, with a flavour quite distinct from all other universities in which I had spent some time. As I discovered, Padua is in fact one of those rare places in which a major university (whose history goes back to the thirteenth century, and is associated with leading figures in European scholarship, from Marsilius of Padua to Zabarella and Galileo) is so well integrated within the social environment of the town that there is not even the memory of significant general clashes between town and gown (a striking difference from Bologna and Oxford). It is, in fact, as if the whole town were a single, large common room, with students and professors all over the place, without there being any obvious attempt by the academic population either to keep a distance (as in Oxford) or to disguise itself (as in Bologna). Students and professors in Padua behave very naturally according to their status and activities, which are obviously well received and highly regarded by the whole population. The Faculty of Statistics, to which I was attached, is a small institution built around Bernardo Colombo, a distinguished demographer, about 20 years ago. There I found myself very much at ease, surrounded by colleagues who often seemed to show a work ethic and research habits more like the natural than the social scientists, with a deep sense of duty and precision but also a vivid perception of academic freedom. In the Economics Department, an establishment quite separate from the Faculty, I met Ferdinando Meacci, an economic theorist who greatly impressed me with his deep and unconventional knowledge of the great authors of our discipline, from Smith and Ricardo to Hayek and Keynes. I found that Ferdinando had a special bent for conceptual distinctions and sophisticated comparisons, a style of thought that sometimes reminded me of certain conversations with Sir John Hicks. We often had lunch and dinner together (sometimes with his wonderful family) and we were able to arrange joint classes on theories of non-proportional dynamics, Hicksian economics, the theoretical foundations of social accounting, and horizontal and vertical theories of economic structure. We also started, with the support of the University of Padua, an international research group on the wealth of nations in economic theory, which has already held a couple of conferences in the beautiful alpine town of Brixen, in south Tyrol.

However, my tenure of the Padua professorship was quite short, since in November 1990 (about a month before the birth of my first child) I moved back to my old Alma Mater, the University of Bologna, where I was appointed to the Economics Faculty.

I feel unable to comment on my present activity there, since this is really the matter of present everyday life.

Notes

1. The concept of 'complex co-operation' was first introduced by Edward Gibbon Wakefield in his commentary to Smith's *Wealth of Nations*. There Wakefield defined complex co-operation as 'such cooperation as takes place when several persons help each other in different employments' (Wakefield, 1835 43, vol. I, p. 26) and contrasted it with 'simple co-operation', which he considered as a type of co-operation taking place 'when several persons help each other in the same employment' (Wakefield, 1835–43, vol. I, p. 26). The distinction between simple and complex co-operation was anticipated by Melchiorre Gioja, who distinguished between *associazione de' travagli* (association of works) and division of labour, and considered the latter as a sophisticated instance of the former (see Gioja, 1815 17, vol. I, pp. 89–91).
2. Vertically hyper-integrated magnitudes permit one to split the overall economic system into non-overlapping subsystems, such that each 'sector' (that is, each particular subsystem) includes the quantities of labour and intermediate products – the 'productive capacity' directly and indirectly necessary in order to produce a given final consumption good as a net output, to maintain in operation the 'circular flow' supporting the production of that consumption good, and to expand such a circular flow at a given rate of growth. (The concept of 'vertically hyper-integrated sector' was first introduced in Pasinetti, 1988, pp. 126 8, as a

generalization of the concept of 'vertically integrated sector', which had been introduced in Pasinetti, 1973, pp. 5–7; the latter concept may be derived from that of vertically hyper-integrated sector by setting the overall rate of growth equal to zero.)

3. The above situation characterizes primitive trade, until 'the increase of industry and trade' separates goods from 'the special subjective valuations attributed to them by individual persons or groups' (Hoyt, 1926, p. 88).
4. As Adam Smith argued, scientific work often proceeds through the discovery of 'an unusual succession of things', so that human imagination finds it difficult to pass 'along such disjointed objects', and the feeling arises 'of something like a gap or interval betwixt them' (Smith, 1980, p. 42; 1st edn 1795). The 'discovery of a connecting chain of intermediate events' (p. 42) is considered to be a distinctive characteristic of structured thought.
5. The above distinction is suggested by John Hicks, in manuscript notes on a copy of Maffeo Pantaleoni's *Erotemi di economia* (Pantaleoni, 1925) belonging to me. Hicks's reference is to p. 73 of the *Erotemi*, in which Pantaleoni distinguishes between *ordo essendi* (order of being) and *ordo fiendi* (order of becoming). I am grateful to Anthony Courakis, Sir John Hicks's literary executor, for permission to refer to the above unpublished material.
6. A natural ranking reflects one or more scalar characteristics of the relevant objects independent of liking or preference. A subjective ranking reflects both a natural ranking and a preference relation based upon relative positions in the natural ranking.
7. A case in point is the relationship between the ranking of technology matrices according to their uniform net output rates (a ranking that is independent of costs and prices and may thus be considered of the 'natural' type) and the ranking of the same matrices according to the 'rentability' criterion (a ranking that presupposes some type of economic judgement) (see Quadrio-Curzio, 1967, 1980, 1990).
8. This volume contains essays by John Hicks, Bernard Schmitt, Maurice Allais, Michael Bacharach, Dieter Helm, Klaus Hennings, Nicholas Georgescu-Roegen, Michael Landesmann, Alberto Quadrio-Curzio, Krishna Bharadwaj, Christopher Bliss, Alberto Quadrio-Curzio and Roberto Scazzieri, and Luigi Pasinetti.
9. Global technology is defined as a combination of two or more technology matrices such that at least one basic commodity is delivered by two or more processes at the same time. Composite technology, on the other hand, is defined as a combination of semi-independent technology matrices related to one another purely in terms of the utilization of one technology's net output in order to start up another technology.
10. The essays in this volume are contributed by Lorenzo Ornaghi, Takashi Negishi, Heinrich Bortis, Michael Landesmann and Roberto Scazzieri, Jean Magnan de Bornier, Harald Hagemann, Michio Morishima, Nicholas Georgescu-Roegen, Mauro Baranzini and Roberto Scazzieri.
11. Our closest collaborators in the journal include Faye Duchin, from New York University; Georg Erber, from the German Institute of Economic Research in Berlin; Harald Hageman, from the University of Hohenheim, Stuttgart; Shinichiro Nakamura, from Waseda University, Tokyo; and Alessandro Vercelli, from the University of Siena.

References

Allais, M. (1943) *À la Recherche d'une Discipline Economique. Première Partie, L'Economie Pure*. Paris: Ateliers Industria.

Aumann, R. J. (1987) Game theory. In J. Eatwell, M. Milgate and P. Newman (eds), *The New Palgrave. A Dictionary of Economics.* London: Macmillan, Vol. 2, 460–82.

Babbage, C. (1832) *On the Economy of Machinery and Manufactures.* London: Charles Knight.

Bacharach, M. O. L. (1970) *Biproportional Matrices and Input-Output Change.* Cambridge: Cambridge University Press.

Bacharach, M. O. L. (1986) The problem of agents' beliefs in economic theory. In M. Baranzini and R. Scazzieri (eds), *Foundations of Economics. Structures of Inquiry and Economic Theory.* Oxford and New York: Basil Blackwell, 175–203.

Bacharach, M. O. L. (1988) Preferenze razionali e descrizioni individuali. In M. C. Galavotti and G. Gambetta (eds) *Epistemologia ed Economia.* Bologna: Cooperativa Libraria Universitaria, 43–63.

Bacharach, M. O. L. (1989) The role of 'Verstehen' in economic theory. *Ricerche Economiche,* **XLIII**, January–June, 129–50.

Bagolini, L. (1975) The topicality of Adam Smith's notion of sympathy and judicial evaluation. In T. Wilson and A. S. Skinner (eds), *Essays on Adam Smith.* Oxford: Clarendon Press, 100–13.

Baranzini, M. (ed) (1982) *Advances in Economic Theory.* Oxford: Basil Blackwell.

Baranzini, M. and Scazzieri, R. (1986a) Knowledge in economics: a framework. In M. Barazini and R. Scazzieri (eds), *Foundations of Economics. Structures of Inquiry and Economic Theory.* Oxford and New York: Basil Blackwell, 1–87.

Baranzini, M. and Scazzieri, R. (eds) (1986b) *Foundations of Economics. Structures of Inquiry and Economic Theory.* Oxford and New York: Basil Blackwell.

Baranzini, M. and Scazzieri, R. (eds) (1990a) *The Economic Theory of Structure and Change.* Cambridge: Cambridge University Press.

Baranzini, M. and Scazzieri, R. (eds) (1990b) Economic structure: analytical perspective. In M. Baranzini and R. Scazzieri (eds), *The Economic Theory of Structure and Change.* Cambridge: Cambridge University Press, 227–333.

Cencini, A. (1986) Review of *Foundations of Economics. Structures of Inquiry and Economic Theory,* edited by M. Baranzini and R. Scazzieri. Oxford and New York: Basil Blackwell. *Revue d' économie politique,* **96**(6), November–December, 710–17.

Davenant, C. (1698) *Discourses on the Public Revenues, and on the Trade of England in Two Parts.* London: James Knapton.

Edgeworth, F. Y. (1881) *Mathematical Psychics.* London: C. Kegan Paul.

Georgescu-Roegen, N. (1969) Process in farming versus process in manufacturing: a problem of balanced development. In G. U. Papi and C. Nunn (eds), *Economic Problems of Agriculture in Industrial Societies.* London: Macmillan, 497–528.

Georgescu-Roegen, N. (1970) The economics of production. *The American Economic Review,* **LX**, 1–9.

Georgescu-Roegen, N. (1971) *The Entropy Law and the Economic Process.* Cambridge, Mass.: Harvard University Press.

Gioja, M. (1815–17) *Nuovo Prospetto delle Scienze Economiche.* Milan: Pirotta.

Gramm, W. G. (1987). Book review. Foundations of economics: structures of inquiry and economic theory. *Atlantic Economic Journal,* **XV**(4), December, 76–84.

Graunt, J. (1662) *Natural and Political Observations* [...] *upon the Bills of Mortality.* Oxford.

Hahn, F. (1978) On non-Walrasian equilibria. *Review of Economic Studies,* **45**, 1–17.

Hicks, J. (1975) The scope and status of welfare economics. *Oxford Economic Papers,* **27**(3), 307–26.

Hicks, J. (1976) 'Revolutions' in economics. In S. J. Latsis (ed), *Method and Appraisal in Economics.* Cambridge: Cambridge University Press, 207–18.

Hicks, J. (1979) *Causality in Economics.* Oxford: Basil Blackwell.

Hicks, J. (1985) *Methods of Dynamic Economics*. Oxford: Clarendon Press.

Hicks, J. (1989) *A Market Theory of Money*. Oxford: Clarendon Press.

Hoyt, E. E. (1926) *Primitive Trade. Its Psychology and Economics*. London: Kegan Paul, Trench, Trubner.

Hume, D. (1987) (1st edn 1753) *Essays. Moral, Political and Literary*, edited and with a foreword, notes, and a glossary by Eugene F. Miller. Indianapolis: Liberty Classics.

King, G. (1936; MS 1696) Natural and political observations and conclusions upon the state and condition of England. In G. E. Barnett (ed), *Two Tracts by Gregory King*. Baltimore: Johns Hopkins University Press, 1–76.

Landesmann, M. and Scazzieri, R. (1990) Specification of structure and economic dynamics. In M. Baranzini and R. Scazzieri (eds), *The Economic Theory of Structure and Change*. Cambridge: Cambridge University Press, 95–121.

Landesmann, M. and Scazzieri, R. (1991a) The production process: description and analysis. Mimeo, Cambridge (SPES Project).

Landesmann, M. and Scazzieri, R. (1991b) Forms of production organisation: the case of manufacturing processes. Mimeo, Cambridge (SPES Project).

McKenzie, L. and Zamagni, S. (eds) (1991) *Value and Capital Fifty Years Later*. Houndmills: Macmillan.

Marschak, J. and Radner, R. (1972) *Economic Theory of Teams*. Cowles Foundation Monograph no. 22. New Haven and London: Yale University Press.

Neumann, J. von (1937) Über ein Oekonomisches Gleichungs-System und eine Verallgemeinerung des Brouwerschen Fixpunktsatzes. In *Ergebnisse eines Mathematischen Kolloquiums*, Vol. VIII, 73–83, Vienna; English translation: A model of general equilibrium. *Review of Economic Studies*, 1945/6, 1–9.

Pantaleoni, M. (1925) *Erotemi di economia*, Vol. I. Bari: Laterza.

Pasinetti, L. L. (1965) A new theoretical approach to the problems of economic growth. In *Pontificiae Academiae Scientiarum Scripta Varia*, no. 28. Vatican City: 571–696.

Pasinetti, L. L. (1973) The notion of vertical integration in economic analysis. *Metroeconomica*, **XXV**, 1–29.

Pasinetti, L. L. (1981) *Structural Change and Economic Growth. A Theoretical Essay on the Dynamics of the Wealth of Nations*. Cambridge: Cambridge University Press.

Pasinetti, L. L. (1986) Theory of value – a source of alternative paradigms in economic analysis. In M. Baranzini and R. Scazzieri (eds), *Foundations of Economics. Structures of Inquiry and Economic Theory*. Oxford and New York: Basil Blackwell, 409–31.

Pasinetti, L. L. (1988) Growing subsystems, vertically hyper-integrated sectors, and the labour theory of value. *Cambridge Journal of Economics*, **12**(1), March, 125–34.

Pasinetti, L. L. (1992) *Structural Economic Dynamics. A Theory of the Economic Consequences of Human Learning*. Cambridge: Cambridge University Press.

Pasinetti, L. L. and Scazzieri, R. (1987a) Structural economic dynamics. In J. Eatwell, M. Milgate and P. Newman (eds), *The New Palgrave. A Dictionary of Economics*. Vol. IV. London: Macmillan, 525–8.

Pasinetti, L. L. and Scazzieri, R. (1987b) Capital theory: paradoxes. In J. Eatwell, M. Milgate and P. Newman (eds), *The New Palgrave. A Dictionary of Economics*, Vol. I. London: Macmillan, 363–8.

Petty, W. (1690) *Political Arithmetick; or, A Discourse Concerning the Extent and Value of Lands, People and Building [etc.]*. London: R. Clavel and H. Mortlock.

Petty, W. (1691) *The Political Anatomy of Ireland*. London: D. Brown and W. Rogers.

Ricardo, D. (1817) *On the Principles of Political Economy and Taxation*. London: J. Murray.

Quadrio-Curzio, A. (1967) *Rendita e distribuzione in un modello economico plurisettoriale*. Milan: Giuffrè.

Quadrio-Curzio, A. (1975) *Accumulazione del capitale e rendita*. Bologna: Il Mulino.

Quadrio-Curzio, A. (1980) Rent, income distribution, order of efficiency and rentability. In L. L. Pasinetti (ed), *Essays on the Theory of Joint Production.* London: Macmillan, 218-40.

Quadrio-Curzio, A. (1986) Technological scarcity: an essay on production and structural change. In M. Baranzini and R. Scazzieri (eds), *Foundations of Economics. Structures of Inquiry and Economic Theory.* Oxford and New York: Basil Blackwell, 311-38.

Quadrio-Curzio, A. (1990) *Rent, Distribution and Economic Structure. A Collection of Essays.* Milan: (CNR–IDSE), *Dynamis* (1/90).

Quadrio-Curzio, A., Manara, C. F. and Faliva, M. (1987) Produzione ed efficienza con tecnologie globali. *Economia Politica,* **IV**, 11-47.

Quadrio-Curzio, A. and Pellizzari, F. (1991) Structural rigidities and dynamic choice of technologies. *Rivista Internazionale di Scienze Economiche e Commerciali,* **XXXVIII** (6–7), June–July, 481–517.

Quadrio-Curzio, A. and Scazzieri, R. (1977–82) *Protagonisti del pensiero economico,* Vols 1–4. Bologna: Il Mulino.

Quadrio-Curzio, A. and Scazzieri, R. (1982a) La formazione delle idee cardine: rivoluzione industriale e economia politica. In A. Quadrio-Curzio and R. Scazzieri (eds), *Protagonisti del pensiero economico. Rivoluzione industriale e economia politica (1817–1848).* Bologna: Il Mulino, 11–38.

Quadrio-Curzio, A. and Scazzieri, R. (1982b) La formazione delle 'idee cardine': struttura produttiva, scambio e mercati. In A. Quadrio-Curzio and R. Scazzieri (eds), *Protagonisti del pensiero economico. Rivoluzione industriale e economia politica (1848–1872).* Bologna: Il Mulino, 11–48.

Quadrio-Curzio, A. and Scazzieri, R. (1983) *Sui momenti costitutivi dell'economia politica.* Bologna: Il Mulino (2nd edn, same publisher, 1985).

Quadrio-Curzio, A. and Scazzieri, R. (1986) The exchange–production duality and the dynamics of economic knowledge. In M. Baranzini and R. Scazzieri (eds), *Foundations of Economics. Structures of Inquiry and Economic Theory.* Oxford and New York: Basil Blackwell, 377–407.

Rae, J. (1834) *Statement of Some New Principles on the Subject of Political Economy.* Boston: Hilliard, Gray.

Scazzieri, R. (1981) *Efficienza produttiva e livelli di attività. Un contributo di teoria economica.* Bologna: Il Mulino.

Scazzieri, R. (1982) Scale and efficiency in models of production. In M. Baranzini (ed), *Advances in Economic Theory.* Oxford: Basil Blackwell, 19–42.

Scazzieri, R. (1983) The production process: general characteristics and taxonomy. *Rivista Internazionale di Scienze Economiche e Commerciali,* **30**(7), 597–611.

Scazzieri, R. (1989) Economic theory as rational reconstruction. *Ricerche Economiche,* **XLIII**, January–June, 40–56.

Scazzieri, R. (1990) Classical traverse analysis. In *Dynamis,* Quaderni dell'Istituto di ricerca sulla dinamica dei sistemi economici (CNR), no 3. In M. Landesmann and R. Rowthorn (eds), *The Dynamics of Capitalist Economies,* Oxford: Oxford University Press.

Scazzieri, R. (1991a) *Funds, Processes and the Structure of Productive Activity.* Discussion Papers Series of the Economics Department of the University of Padua, no. 7. In P. O'Sullivan (ed), *Beyond the Austrian School,* London: Macmillan.

Scazzieri, R. (1991b) Elementi per una teoria dell'ordine economico. In C. Galli (ed), *Logiche e crisi della modernità.* Bologna: Il Mulino, 269–88.

Scazzieri, R. (1992a) *A Theory of Production. Tasks, Processes and Technical Practices.* Oxford: Clarendon Press.

Scazzieri, R. (1992b) Economic theory and economic history: perspectives on Hicksian themes. In H. Hagemann and O. Hahouda (eds). *The Legacy of Hicks.* London: Routledge.

Smith, A. (1976; 1st edn 1776) *An Inquiry into the Nature and Causes of the Wealth of Nations*. R. H. Campbell and A. S. Skinner, general editors, W. B. Todd, textual editor. Oxford: Clarendon Press.

Smith, A. (1980; 1st edn 1795) 'The principles which lead and direct philosophical enquiries; illustrated by the history of astronomy', in W. P. D. Wightman and J. C. Bryce (eds), *Essays on Philosophical Subjects*. Oxford: Clarendon Press.

Sraffa, P. (1925) Sulle relazioni fra costo e quantità prodotta. *Annali di Economia*, **II**, 277–328.

Sraffa, P. (1932) Dr Hayek on money and capital. *The Economic Journal*, **42**, 42–53.

Sraffa, P. (1960) *Production of Commodities by Means of Commodities. Prelude to a Critique of Economic Theory*. Cambridge: Cambridge University Press.

Steuart, J. (1966; 1st edn 1767) *An Inquiry into the Principles of Political Oeconomy*. Edinburgh/London: Oliver and Boyd for the Scottish Economic Society.

Urmson, J. O. (1950) On grading. *Mind*, **LIX**, April, 145–69.

Wakefield, E. G. (1835–43) Commentary. In *An Inquiry into the Nature and Causes of the Wealth of Nations*, by Adam Smith, LLD, with a commentary by the author of *England and America*. London: Charles Knight.

Wright, G. H. von (1963) *Norm and Action. A Logical Enquiry*. London: Routledge and Kegan Paul.

Wright, G. H. von (1980) *Freedom and Determination*. Helsinki: The Philosophical Society of Finland.

5

Reinhard Selten

In search of a better understanding of economic behaviour

The title of this chapter tries to express a leading theme in my scientific efforts. Most of my past work can be seen as an attempt to explore the structure and the consequences of economic behaviour. At least this is true if the term 'economic behaviour' is understood in a sufficiently wide sense to include applications of game theory to political science and evolutionary biology.

I do not want to convey the false impression that my research is single-mindedly organized around a grand question. I am easily attracted by the opportunity to shift my interests into unforeseen exciting new directions. The little coherence there is in my work is due to a desire to understand both fully rational and boundedly rational economic behaviour, especially in the context of game situations.

1. Early influences

In my high-school years I developed a strong interest in mathematics. My first exposure to economic theory was a history of economic thought by Heimann which I happened to find in a public library. Under the impression of this book, I shortly after considered studying economics, but in the end I did not change my original plans. In 1951, I began to study mathematics at the University of Frankfurt-on-Main.

As a student I did not work as hard as I should have done. I spent a lot of time listening to lectures and reading books in fields not directly connected to my studies. I had to take physics as subsidiary, a subject which did not interest me as much as economics and psychology. At this time the well-known *Gestalt* psychologist Edwin Rausch was teaching at Frankfurt. I went to his lectures and served as an experimental subject for a sufficient number of hours to be admitted to one of his proseminars. Later, it turned out that the time I spent on the study of experimental psychology was by no means wasted. Of course, the same is true of my early efforts to learn some economic theory. However, I also extended my curiosity to other fields such as astronomy and the history of science which at least up to then did not supply useful background knowledge for my scientific work.

Even before I began to study, I knew of the existence of an interesting new field called game theory about which I had read an article by Williams in *Fortune*. At the university, I read the book by von Neumann and Morgenstern (1944) in order to learn more about the subject. At first this also seemed to be an extracurricular effort, since nobody taught game theory at the Mathematics Department. To my surprise, somewhat later I discovered the announcement of a student seminar on game theory for economics students offered by Ewald Burger as a part of his teaching of mathematics for economists. I participated in this seminar and gladly followed the suggestion of Ewald Burger to write a master's thesis on the problem of defining a solution for general two-person games in normal form.

Burger later published an important textbook on game theory (1958) which has been translated into English (1963). It is worth looking at even today after so many years for its rigorous and concise proofs for the existence of equilibrium points in finite and infinite normal form games. He was an excellent teacher who was always willing to interact with his students. I owe much to his guidance.

2. Valuation of games

The work that I did under the supervision of Burger, first for my master's thesis and later for my PhD thesis, was an axiomatic value theory for extensive side payment games (1960, 1964). I first tried to base my theory heavily on a subgame truncation axiom which postulates that the value of a game remains unchanged if a subgame is cut off and replaced by its value as the pay-off vector at the decomposition point. I succeeded with this approach in the two-person case but soon discovered that for more than two players the subgame truncation property is incompatible with other plausible axioms. I obtained these results in my master's thesis. In my PhD thesis, finished in 1961, I presented an axiomatic value theory for n-person games. The English translation was published only four years later.

My work on value theory was in the spirit of the time. The problem of defining rationality in co-operative games was foremost in the minds of game theorists. In view of a popular belief that co-operation must take place wherever it is profitable, non-co-operative games seemed to be much less interesting.

My value theory is closely related to Harsanyi's general solution theory for co-operative games (1963). For the side payment case, to which my value is restricted, both theories come to the same conclusions, but from different points of departure. His work is based on a bargaining model. I avoided the interpretation of my value by a bargaining model, since I was disturbed by examples of three-person games, in which the threat pay-off of a player against the coalition of the two others is higher than his value.

3. The beginnings of experimental economics in Germany

After I had finished my master's thesis in 1957, I was hired by Heinz Sauermann who held a Chair of Economics at the University of Frankfurt-on-Main. He knew me as a participant of a colloquium organized by Burger, in which economists and mathematicians discussed applications of mathematics to economics. He must have been positively impressed by several talks I had given in this colloquium. He was one of the very few German economists who at this time already clearly saw the trend towards the use of more and more mathematics in economic theory. In spite of his lack of mathematical training, he was in favour of this development.

I worked under Heinz Sauermann for ten years in various assistant positions. At first it was my task to do research on 'Application of decision theory to the theory of the firm', a project financed by the Deutsche Forschungsgemeinschaft (the German counterpart of the National Science Foundation). I actually began to do something which fitted the name of the project. I tried to show that under certain assumptions on the economic environment, maximal long-run growth of the firm is achieved by the maximization of the expected logarithm of the firm's assets. However, I very soon abandoned this effort. H. A. Simon's pioneering work in his book *Models of Man* (1957) convinced me of the necessity to model economic behaviour as boundedly rational. Suddenly the application of normative decision theory to the theory of the firm seemed to be pointless.

Even before I was converted to Simon's views, I began to pursue the plan to perform oligopoly experiments. I had read a little book by Ricciardi (1957) which described the first computerized management game used by the American Management Association as a training device. It occurred to me that a much simpler business game could serve as an experimental research tool. No computer was necessary for this purpose. The idea to perform experiments was also due to the example of the pioneering work of Kalish *et al.* (1954), whose paper on the first characteristic function game experiments had impressed me very much.

At this time experiments in economics were almost unheard of. It was a standard textbook assertion that experiments are impossible in economics. I was lucky to work under the supervision of Heinz Sauermann, who had the courage and foresight to support my unusual plans and to get involved in experimental work himself. My first paper on oligopoly experiments (1959) was a joint work with him. He became a great supporter of experimental economics. He organized a number of successful conferences and edited a series of books, the *Contributions to Experimental Economics.* The first volume of this series (1967) collected experimental work done under his supervision at the University of Frankfurt in the time from 1959 to 1965.

Our paper of 1959 was very early, but it was by no means the first contribution to experimental economics. A bibliography compiled by Volker Häselbarth (1967) in the first volume of *Contributions to Experimental Economics* lists 20 publications before 1959. However, it can be said that our little group at Frankfurt was not preceded by any other group doing continuous research on experimental economics over many years. Experimental economics as a field of economic research did not emerge before the 1960s.

Our experimental work was not primarily directed at the testing of existing theories. We tried to build up interesting economic situations in the laboratory in order to gain insight into the structure of boundedly rational decision-making. The exploratory style of our research and our emphasis on the explanation of individual behaviour were not characteristic of the emerging field as a whole. In the USA, there was more interest in effects on the level of the system rather than the individual and the evaluation of experimental data was often guided by an unwavering confidence in the explanatory power of strong rationality assumptions. To some extent, this is still true even now.

4. A step on the thorny path towards a theory of bounded rationality

It is now clear to me that it will take many decades of painful experimental research until an empirically defendable general theory of bounded rationality emerges. At the beginning of my career, I was more optimistic in this respect. Together with my teacher Heinz Sauermann, I tried to develop a general theory of boundedly rational decision making of the firm. I had an idea how to model the search for alternatives and the final choice as a process guided by rules for the adaptation of aspiration levels on multiple goals. Finally, a complete theory emerged in long discussions with Heinz Sauermann. He was not only a professor of economics but also the owner of a textile wholesale business. His practical experience was the empirical background for our common work. We finally wrote a paper in German (1962) with the title 'Anspruchsanpassungstheorie der Unternehmung' (Aspiration-level adaptation theory of the firm).

This paper described a formal structure, but without any formulas. In this way, we hoped to reach a broad audience. Our theory never became known outside the German language area, but it seems to me that it still has something to offer as a point of departure for further work. At least we succeeded in constructing a coherent and plausible picture of non-optimizing decision making based on rules for the adaptation of aspiration levels on incomparable multiple goals. The question of optimization does not even arise in our theory, since it does not specify preferences among potential final choices, but only local priorities for the adaptation of aspiration levels. Bounded rationality is not conceived as an approximation to full rationality but as something different.

In the light of experimental evidence, I later came to the conclusion that boundedly rational behaviour is more complex than our theory of aspiration level adaptation suggests. We took goals, aspiration scales and adaptation rules as given, in the same way as preferences are taken as given by orthodox decision theory. I now think that, at least in some contexts, aspiration levels are constructed from more basic data. We need a theory on how this is done.

Our attempt to build a general theory of boundedly rational decision-making of the firm was, perhaps, premature. It was not more than a step on the long and thorny path towards an empirically sound theory of bounded rationality.

5. Causal diagrams

My second experimental paper on oligopoly (1967a) dealt with a model in which three firms had to choose capacities and production methods. Production was equal to capacity and the price was a linear function of total supply. The firms, represented by groups of three to five subjects, had to write protocols explaining the reasons for their decisions. Inspired by the influential paper on psycho-logic by Abelson and Rosenberg (1958) who had modelled political belief structures by signed graphs, I interpreted the protocols by graph structures which I called 'causal diagrams'. Formally, a causal diagram is a signed directed graph whose nodes correspond to variables and whose links represent positive or negative causal influences.

A causal diagram is 'balanced' if all causal chains from a decision parameter to a goal variable show an influence in the same direction. Subjects tend to achieve balance by the suppression of causal links implied by the rules of the game. This results in 'simplified' causal diagrams.

On the basis of the simplified causal diagrams, the firms determine the order in which capital widening and capital deepening investments are made. I called this order the 'strategic line'. Investment criteria determine when the next step on the strategic line is taken. The investment criteria have the character of aspiration levels on profit-related variables.

Similar qualitative belief structures were later explored by Axelrod (1976) under the name of 'cognitive maps'. It seems to be the case that many decisions are made in two stages. First, in a qualitative stage, it is decided which decision parameters should be changed in which direction. Second, in a quantitative stage, the amounts of change are determined. The qualitative decisions of the first stage are made by qualitative reasoning on the basis of qualitative information. Different principles, e.g. a conventional step size of change, are applied in the second stage.

6. Equal shares and coalition formation

In the early 1960s, a young psychologist, Klaus G. Schuster, joined our little research group at Frankfurt University. Together with him, I performed experiments on the five-person apex game, a special characteristic function game (1968). Listening to the face-to-face bargaining among the subjects, I had the impression that equal shares of coalition values have a great significance for the thinking of the players. Kalish *et al.* (1954) had already made a remark to that effect. However, since I was not able to substantiate my impression by a statistical analysis of the result of our experiments, we did not mention it in our paper.

I had the idea that players would tend to form a coalition with maximal equal share and that the agreed-upon pay-offs would be determined by aspiration levels derived from maximal equal shares of alternative coalitions. Within the region delineated in this way, I expected a numerically prominent pay-off division. Based on this principle, I was surprisingly successful in the prediction of classroom demonstration experiments on a specific seven-person game. For many years, I tried to generalize my tentative theory to all superadditive characteristic function games. For this purpose, I looked very hard at all published results of characteristic function experiments. I compiled a list of 211 plays. This data base was too small for a theory which specifies an explicit process of aspiration level formation. However, I found out that a simple theory called 'equal share analysis' fitted the available data quite well (1972). This theory does not assert that coalition values are split evenly. Unequal divisions are possible and may even be required. However, equal divisions of alternative coalition values bound the pay-off division within a coalition. Pay-off vectors must be in the 'equal division core', which means that no equal division of an alternative coalition value improves the pay-offs of all members.

Equal division is not only a fairness norm but also a natural point of departure for boundedly rational strategic thinking. A coalition member who is stronger than his partners has a reason to claim at least his equal share.

Equal share analysis was a better explanation of the data than its alternatives proposed by normative game theory, but it was not really satisfactory. As more data became available, I developed a new descriptive theory for zero-normalized superadditive three-person games in characteristic function form. This theory, called 'equal division pay-off bounds' (1983c, 1987), derives lower bounds for the players' aspiration levels based on simple computations involving various equal shares. The improved version of this theory (1987), in particular, has had a remarkable predictive success. A student of mine, Gerald Uhlich (1990), has proposed a generalization and modification called 'proportional pay-off bounds' which is applicable to three-person games with non-negative one-person values.

The descriptive theories developed by Uhlich and me are procedural in the sense that they specify the way in which the solution is determined. Moreover the theories are casuistic in the sense that many case distinctions based on simple criteria are made: simple principles are applied in every single case. Casuistic procedural structures seem to be more adequate for the description of boundedly rational coalition formation than solution concepts based on abstract general principles.

7. Presentation effects

Together with Claus C. Berg (1970), I performed oligopoly experiments in which we varied initial assets and profits in a way which did not change the relationship between strategic choices and final money pay-offs. Increased initial assets were compensated by increased fixed costs. This strategically inessential variation of the rules of the game had a marked influence on behaviour. One of the modes of observed co-operation was Pareto-optimal equal profits, a fairness concept which is not invariant to changes of fixed costs compensated by changes of initial assets. We called this a 'presentation effect'. In the terminology used now one would speak of 'framing' (Kahneman and Tversky, 1984). We were among the first to describe a phenomenon of this kind. Therefore I feel that I should be permitted to continue to speak of presentation effects. Another early description of a presentation effect can be found in a study of Pruitt (1970) who showed that the way in which a prisoner's dilemma game was presented to the subjects made a dramatic difference to the frequency of co-operation in repeated play.

Presentation effects are the most convincing evidence against utility maximization as an explanation of observed behaviour. In the case of the oligopoly experiment described above, the presentation effect becomes easily understandable if one thinks of behaviour as guided by aspiration levels. Zero profits are an obvious focus for the formation of a first aspiration level on pay-offs. If aspiration levels are formed in this way, equal profits are a natural mode of co-operation.

8. From oligopoly experiments to subgame perfectness

At the University of Frankfurt, I did experimental work on characteristic function bargaining (Selten and Schuster, 1968) and bilateral markets (Selten, 1970b), but most of my experimental research was concentrated on oligopoly. I was fascinated by the problems posed by the task of playing a business game. Usually the participants must reach their decision in a relatively short time. Obviously they cannot apply Bayesian decision theory or game theory. How should they approach their task? In this respect economic theory miserably fails to offer practical advice.

In my view it is of great importance to gain theoretical insight into the problem. With my work on oligopoly experiments, I tried to contribute to the accumulation of an empirical background for the development of a theory of boundedly rational decision making in business games. I think that it is worthwhile to work towards this goal even if we are still far away from it.

Even if the behaviour of experimental subjects cannot be expected to conform to normative game-theoretical solutions, it is useful to determine such solutions if this is possible. The way in which observed behaviour deviates from normative theory often conveys valuable insights into the structure of boundedly rational decision making.

One of the oligopoly experiments performed at Frankfurt (1967b) was based on a dynamic price variation model with demand inertia. When I constructed the model, I did not pay any attention to analytic tractability. The model has indivisibilities of production and investment and interest rates are different for positive and negative accounts. Only later it occurred to me that a game-theoretical analysis might be feasible for a simpler version of the model without these features. I determined an equilibrium solution by backward induction. Having done this, I discovered the presence of many other equilibrium points in pure strategies. However, the backward induction equilibrium was clearly the natural non-co-operative solution. In order to describe its distinguishing features in general game-theoretical terms, it was necessary to introduce the notion of subgame perfectness.

The paper was written in German (1965) and probably has not been read by many people even if it is quoted quite often for the definition of subgame perfectness. Experiments on the oligopoly model with demand inertia analysed in this paper were not performed until much later and not by me, but by my PhD student Claudia Keser (1992). Needless to say, the subgame perfect equilibrium solution did not prove to be a valid descriptive theory. I never had this irrational expectation.

9. My book on the multiproduct firm

When I began to work under the supervision of Heinz Sauermann, most German economists thought that mathematics had no future in economics. People like me, who entered economics by a side-door with a background in mathematics, were looked upon with suspicion. Therefore, I was first expected to acquire a master's degree and then a PhD in economics. I am grateful to Heinz Sauermann who made it possible for me to avoid these steps in my career. Several years after I had received my PhD in mathematics, he must have succeeded in convincing his colleagues that I should be permitted to write a habilitation thesis in economics without meeting the usual prior requirements. In Germany the habilitation, not the PhD, is the last formal barrier to eligibility for professorship. Heinz Sauermann asked me to write a habilitation thesis which would also be of interest to his colleagues teaching business administration. I decided to work on the multiproduct firm.

When I made this choice, I knew nothing about the literature on the subject-matter. What attracted me to the area was a strong interest in the theory of the firm combined with an inclination towards problems involving an unspecified integer number *n* in an essential way like the algebraic equation of *n*th degree which captured my imagination in my high-school days. I was not able to do anything about the *n*th degree equation, but I had successfully worked on the valuation of *n*-person games. I felt that it would be interesting to do something about the *n*-product firm.

Reading the literature, I was surprised by the unsuspected richness of the area. I learnt about the Edgeworth paradox of a monopoly price reducing excise tax and the phenomenon of the loss leader optimally sold below marginal cost. The classical German habilitation thesis is a monograph on a well-defined subject area. My book on the multiproduct firm (1970a) conformed to this pattern. I systematically represented the traditional theory of the multiproduct monopoly and extended it where I could. In addition to this I also developed a theory of Cournot Nash equilibrium in multiproduct oligopoly. My special emphasis was on problems of aggregation. The problem of determining optimal prices for the multiproduct monopoly with linear demand and quadratic cost can be reduced to solving a system of three linear equations for three price indices, if the interrelationships of cost and demand are sufficiently unsystematic in a certain sense. A set of conditions on multiproduct oligopoly models permits an analysis in terms of an aggregated decision parameter called 'aggressivity'.

10. Bargaining under incomplete information

In 1965, I was invited to a workshop on game theory in Jerusalem. Only 17 participants attended this conference: game theory was still a very small field. The hottest topic of discussion was Harsanyi's new theory of games with incomplete information (Harsanyi, 1967–8). I supported Harsanyi's idea that it is natural to require a consistency condition on the players' beliefs which makes it possible to model incomplete information by imperfect information on a lottery performed by nature before the beginning of the game. The consistency condition was heavily criticized and the question arose of how to reduce incomplete to imperfect information without it. I contributed the idea to do this with the help of lotteries performed at the end. This was the beginning of a long co-operation with John C. Harsanyi.

Following his suggestion, the University of California, Berkeley, invited me for the academic year 1967–8 as a Visiting Full Professor at the business school. We worked on a theory of two-person bargaining under incomplete information (Harsanyi and Selten, 1972). It became more and more clear to us that a full understanding of rational co-operation requires non-co-operative modelling.

On the basis of our common paper, I later worked out a semi-numerical example (1975b). Moreover, I began a co-operation with Austin Hoggatt.

Together with several of his research assistants, we performed experiments on what was essentially a special case of this example (Hoggatt *et al.*, 1978). Earlier than others Hoggatt had built up a computerized laboratory. He did this at a time when computers were very primitive compared to the technology available now. Writing the software for his laboratory required great skill and a tremendous amount of work. The results of our experiments were quite interesting. As the theory predicts, conflict usually occurred if both players were in a strong bargaining position, but, of course, observed behaviour was only boundedly rational.

11. Game models of international relations

In the late 1960s, I belonged to a group of game theorists hired by the research firm Mathematica to work on projects for the Arms Control and Disarmament Agency. We often met for several days at Washington DC or nearby. My common work with John Harsanyi on bargaining under incomplete information was also partly done in the framework of this group.

Herbert Scarf, who also was one of the members, proposed a model of nuclear deterrence which had the structure of a co-operative game whose stability could be explored with the help of core concepts. Together with Reinhard Tietz (1972), I developed a theory of 'security equilibria' for a modified version of this model. This theory involves aspects of bounded rationality and can be described as a hybrid of maximin and equilibrium theory for a game form akin to an extensive form. It is assumed that no player does anything which decreases his security level and that at every situation in the game a player's security level is the worst that can happen to him under this assumption.

With the help of numerical computations of security equilibria for special cases of the model, interesting results could be obtained which revealed reasons for stability and instability of nuclear deterrence systems. Neither the work of Scarf nor that of myself and Tietz was very well received in a presentation to the Arms Control and Disarmament Agency. Some of my difficulties were terminological. In the description of the pay-off functions I insisted on talking of a 'motive of revenge', even if in the audience there was strong resentment against the word 'revenge'. Somebody said that, maybe, the Soviet Union has a motive of revenge, but not the USA, since this is against Christian ethics – retaliation, yes, but not revenge. Stubbornly, I refused to talk about retaliation instead of revenge.

I had the impression that the practitioners did not really like to listen to anything applied. They felt they knew best how to solve their problems. They were much more pleased by qualitative interpretations of abstract game-theoretical results. Robert J. Aumann gave an excellent talk of this kind. He explained under what conditions it is advantageous to reveal or not to reveal information by strategic actions in repeated games with incomplete information.

I have been told that after Aumann's presentation a high official said to his entourage: 'We know these things, but we like to be reminded.'

My experiences with the Arms Control and Disarmament Agency did not discourage my inclination towards the theory of international relations. During one of my frequent visits to Berkeley, I met Amos Perlmutter, a political scientist, in the home of John Harsanyi. We decided to co-operate on the application of game theory to the theory of international relations. We developed a method called 'scenario bundle analysis' of constructing simple game models on the basis of expert judgements. Financial support by the Volkswagen Foundation enabled me to organize a small conference of area specialists on potential conflicts in the Persian Gulf. In this conference we applied our method. The participants had to work out various lists such as a list of actors, and for each actor lists of goals, fears and initial options. On the basis of qualitative information of this kind 'scenario bundles' were worked out. Scenario bundles are simple game structures, essentially games with perfect information, but with possibilities of coalition action.

Unfortunately, the report about the results of the conference (1977) was never formally published, mainly because our experts judged an Iranian revolution as extremely improbable. To my surprise, very soon this event happened and made our analysis obsolete. Nevertheless I think that our method is not without merit. It is useful for a chess player to analyse his situation before he makes his choice, even if the other player may easily respond with an unexpected move. I also think that our way of looking at the behaviour of governments as motivated by historically grown goals and fears rather than by an abstract objective such as power or national interest is the right approach to the modelling of concrete international conflict.

Several years ago, Peter Bernholz asked me to co-operate in a joint summer course on international relations at the Austrian College at Alpach. For this occasion I developed a parlour game with the intention to create an environment in which the traditional balance of power theory could work. The game is not without entertainment value. Up to 12 countries form changing alliances in a world of 30 provinces. A stationary equilibrium solution can be determined for a simplified version with only 6 provinces (1991c).

I am a supporter of the international language Esperanto and like most other Esperantists I would like to see the world peacefully united. Therefore I have a special interest in the theory of international relations. A better understanding of war and peace in an obsolete system of nation states may help to bring us nearer to a true world federation.

12. Oligopolistic general equilibrium

During frequent visits to Berkeley, I co-operated with John Harsanyi, Austin Hoggatt and Thomas Marschak. My joint work with Thomas Marschak resulted in a book (1974), *General Equilibrium with Price Making Firms*. In this book and in two articles (1977, 1978) we proposed a model of oligopolistic general equilibrium which took its point of departure from the German tradition of conjectural oligopoly theory. Expected responses to deviations from a status quo are described by 'reaction functions' and stability is defined by the absence of potential deviations which improve profits after the reaction of the competitors. We imposed a rationality criterion on the system of reaction functions which requires restabilizing reactions after a deviation. In this way we arrived at the concept of a 'convolution'. We chose this name for our solution concept because it was suggested to me by Robert J. Aumann in a dream.

Our theory describes oligopolistic competition by a specific convolution which combines aspects of kinked demand curve theory and limit pricing. The firms are assumed to have limited information about the whole economy. They form models on the basis of this information in a systematic way and stability is defined with respect to the reaction of the competitors in the model. It is possible to prove the existence of stable states for the whole economy.

Even if our book is often mentioned in the literature, it did not have as much influence on later work about general equilibrium with imperfect competition as we had hoped for. However, the problem area is still wide open.

13. Multistage game models of imperfect competition

In the early 1970s, I began to construct game models of imperfect competition, in which the competitors make different types of decisions sequentially in several stages. Later, multistage game models of this kind became more and more common in the new industrial economics literature.

My first multistage game model (1973) was an attempt to solve a basic problem of oligopoly theory. It is a widely held belief that the intensity of competition crucially depends on whether or not the number of competitors is small. The competition among few is believed to be qualitatively different from that among many, but where is the dividing line? I constructed several models in order to answer this question and finally worked out what seemed to be the most interesting one.

The model is a three-stage game. At the first stage, players decide whether they want to take part in cartel negotiations. At the second stage, quota cartels can be formed by unanimity bargaining and at the third stage production quantities are chosen. I analysed this game with the help of a selection theory which determined a unique 'perfect equilibrium set'. The solution shows that in this model four are few and six are many. From four to six, the probability of forming a cartel drops from 100 per cent to less than 2 per cent. This is due to a sudden increase in the incentive to become an outsider.

Since I wished to bring my theoretical thinking into closer contact with stylized empirical facts, I engaged in a systematic search for empirical regularities described in the industrial organization literature. I hoped to find an integrated explanation of as many of them as possible. The result of this effort was my paper, 'Oligopolistic size structure and profitability' (1983a), which presents a two-stage game model with entry and choice of technological level at the first stage and production quantity decisions at the second.

The model explains four empirical regularities: the typical skewed distribution of firm sizes in a market, the positive correlation between firm size and firm profitability, the positive correlation between concentration and market profitability and the negative one between marginal concentration and market profitability. Contrary to conventional explanations as direct causal relationships, the validity of the last three relationships in the model is due to the structure of the equilibrium and its response to changes of cost and demand parameters.

A third multistage game model (1984) permits the conclusion that under plausible conditions the prohibition of cartels increases the total profit of all firms in the economy. The effect is due to excessive entry into collusive markets.

Since I do not believe in the descriptive relevance of strong rationality assumptions, I prefer to think of game equilibrium in empirically oriented models as the result of adaptive dynamic processes. In principle, the justifiability of an interpretation in these terms should be explicitly discussed in every single case. Unfortunately, we do not yet have sufficiently well-developed theoretical tools for this purpose.

14. Perfectness

Soon after I had introduced subgame perfectness (1965), it became clear to me that more than that is needed in order to exclude equilibrium points which specify unreasonable behaviour at unreached information sets. I had some ideas how to solve this problem by a concept similar to sequential equilibrium (Kreps and Wilson, 1982b), but I saw no way to prove existence. I therefore abandoned this approach in favour of the trembling hand definition (1975a).

I regard perfectness as a necessary, but not sufficient, rationality requirement for equilibrium points in extensive games. Many stronger refinement concepts have been proposed in the literature. The book by van Damme (1987) provides an overview and a thorough discussion. In my view the stronger concepts go beyond the establishment of a minimal rationality criterion. Trembling hand perfectness is based on the idea of independent local mistakes. Nothing is assumed about the likely reasons for such mistakes. Stronger refinement concepts seem to imply the idea that some deviations can be excluded as less rational than others. Thus it is the essence of simple forward induction arguments that a player is unlikely to make a local mistake which results in playing a dominating strategy in the whole game.

If an equilibrium point is the solution of the game, then every deviation from it is non-rational. Therefore I see no justification for rationality arguments as a basis for excluding some deviations as unlikely. Of course, these short remarks cannot do full justice to the refinement discussion, but they may serve to indicate my point of view. Even if I admire the mathematical and conceptual ingenuity behind some of the stronger refinement concepts, I see no reason to think of perfectness as superseded by one of them.

15. Equilibrium selection

After we had finished our paper on bargaining under incomplete information (1972), John Harsanyi and I thought about extending this work from fixed threats to variable threats. However, we decided that we should first try to solve the general problem of selecting a unique equilibrium point for every non-co-operative game. We would then have a better basis for the treatment of the variable threat case of bargaining under incomplete information. We did not expect that it would take us 18 years to complete our work on equilibrium selection.

We discarded two fairly well-developed theories before we began to build our third one, the final version of which is presented in our book (1988). Each theory was based on a different concept of risk dominance of one equilibrium point over another. The first theory took its point of departure from what we called 'diagonal probabilities'. Suppose that all players deviate with the same probability p from one equilibrium point u to another equilibrium point v. Player i's diagonal probability for u is the maximum value of p such that player i's best reply to the resulting mixture is still his strategy at u. At first we considered a measure of risk dominance formed by the product of the diagonal probabilities at u divided by the product of the diagonal probabilities at v. Later we decided to modify the factors by exponents derived in a complex way as solutions of systems of linear equations. Finally we obtained a selection theory which seemed to have good properties but was practically unmanageable in the application to simple examples.

The risk dominance concept of our second theory and that of our final theory have a basic idea in common which leads to a prior probability distribution over the behaviour of the players. A player assumes that the others know whether u or v is the solution and that they will behave accordingly. He has a subjective probability p for u being the solution and maximizes his pay-off on the basis of this expectation. The subjective probability p is a random variable. In our final theory we always assume that p is uniformly distributed over the unit interval. In our second theory we also considered biased distributions obtained as follows. A public announcement of the solution by a game theorist who is known to be right with a probability r, transforms the initial subjective probability p to a new one by Bayes's rule. Risk dominance of u over v was measured by the size of the 'bias' r needed to bring the prior probability distribution into a 'formation' containing u, but not v. A formation is defined as a substructure closed with respect to best replies.

Our second theory was less unwieldy than our first one, but it still had the difficulty that in special cases risk dominance comparisons alone would not single out a unique equilibrium point. In such cases we had to rely on a very complicated tie-breaking procedure.

Harsanyi's invention of the tracing procedure (1975) made it possible to abolish the tie-breaking procedure and to reconsider the definition of risk dominance and its role in the process of equilibrium selection. Starting from an arbitrary mixed strategy combination interpreted as a prior distribution, the tracing procedure determines a unique equilibrium point.

In our first two theories, a cardinal measure was associated with risk dominance, but finally we decided to define risk dominance as an incomplete binary relation. In order to determine the risk dominance relationship between two equilibrium points, the tracing procedure is applied to the unbiased prior probability distribution.

The final version of our third theory proved to yield intuitively satisfactory results in all applications explored up to now, not only those included in our book but also others published elsewhere (Selten and Güth, 1982, 1991; Leopold, 1982, 1985; Selten and Leopold, 1983; Avenhaus, Güth and Huber, 1991; van Damme and Güth, 1991; Güth, 1991; Güth and Selten, 1991; Potters, van Winden and Mitzkewitz, 1991).

Each of our three theories was changed again and again as the result of heated discussions. Many ideas were proposed and many were discarded. It is only human that both of us wanted to exert as much influence as possible on the common product. However, both of us were willing to yield to stronger arguments at the end.

Probably some of the ideas which we discarded will reappear in later work on equilibrium selection. To some extent this has already happened. Güth and Kalkofen (1989) made use of diagonal probabilities, but their proposals are very different from our first theory.

It can be expected that the long discussion between John and me is only the prelude to a much longer public discussion with many participants. The problem of equilibrium selection by rational players is of great philosophical significance in spite of the limited descriptive relevance of rational game theory.

16. Non-co-operative characteristic function bargaining

Motivated by my experimental interests, I tried to find a non-co-operative procedure for characteristic function bargaining whose game-theoretic analysis yields conclusions akin to classical co-operative game theory. My first attempt in this direction was a simple infinite perfect information model (1981). At the beginning an 'initiator' is selected randomly. An initiator can propose a pay-off division for a coalition and name a member as the next 'responder'. If a responder rejects the proposal he becomes an initiator. If he accepts he has to name a next responder among those members other than the proposer who have not yet accepted the proposal. If there are no such players the coalition is formed and the game ends. An initiator can also shift the initiative to another player.

The game-theoretical analysis exhibited a close correspondence between stationary subgame perfect equilibria with certain additional properties and the stable demand vectors proposed by Albers (1975). For three-person quota games without the grand coalition or with an empty core this means that a two-person coalition with pay-offs according to the quotas must be formed. Experiments with the model do not confirm this prediction (Uhlich, 1990).

The model is an infinite game and strictly speaking only finite games can be played in the laboratory. Another difficulty with the model is posed by the presence of many additional non-stationary equilibrium points. For these reasons I constructed a finite model of characteristic function bargaining which yields quota agreements in the case of three-person quota games without the grand coalition as the only subgame perfect outcomes (1992). In this 'demand commitment model' players may temporarily commit themselves to pay-off demands or they may form coalitions with players who are already committed.

I am looking forward to experiments with this model. Here classical co-operative theory seems to have its best chance. Preliminary experiments with a similar but different model suggest that quota agreements may in fact have a chance to be learnt under favourable experimental conditions, even if up to now they were not typical as outcomes of characteristic function experiments.

17. Evolutionary game theory

The subject-matter of evolutionary game theory is applicable to biology. Luckily, I came into contact with this new field quite early, only about five years after the publication of the pioneering article by Maynard Smith and Price (1973). At that time I was attached to the Institute of Mathematical Economics at the University of Bielefeld. There, a unique institution, the Centre for Interdisciplinary Research, provides unusual opportunities for scientific contacts among people with very different backgrounds. Since I have always liked to look over the fence of my discipline, I made ample use of these opportunities. I became acquainted with many colleagues from other departments. One of these colleagues, the biologist Hubert Hendrichs, once approached me and told me that game theory is now applied to biology and that he would send me one of his students who knew more about this.

The student was Peter Hammerstein, a young mathematician who was working towards a PhD in biology. He explained to me the above-mentioned paper by Maynard Smith and Price (1973). This was the beginning of a long collaboration which is still continuing. In 1978, a conference on evolutionary game theory was held at the Centre for Interdisciplinary Research at Bielefeld. Hammerstein was one of the organizers of this excellent conference. John Maynard Smith and other leading researchers in the newly emerging field were present. Stimulated by exciting talks and lively discussions, an idea occurred to me about how to prove that evolutionarily stable strategies must be pure in two-player game models of animal conflicts with incomplete information in which the opponents always have different roles. Maynard Smith encouraged me to submit the paper (1980) to the *Journal of Theoretical Biology*.

The definition of an ESS (evolutionarily stable strategy) by Maynard Smith and Price is not well adapted to games in extensive forms. If this definition is applied literally, even very simple extensive game models with perfect information do not have any ESS. In order to overcome this difficulty, I developed a weaker concept of a limit ESS (1983b). For this purpose I made use of the same idea of small mistakes which underlies the definition of perfectness. However, in the new context I permitted such mistakes, but did not require them. Curiously enough, essentially the same apparatus which strengthens the notion of equilibrium to perfect equilibrium, weakens the notion of an ESS to that of a limit ESS.

Unfortunately, my paper (1983b) contains a serious mistake concerning decomposition into subgames and truncations. It is bad enough that I did not avoid an elementary error, but it is even worse that my intuition was wrong. I should have seen that an ESS does not decompose in the same way as a perfect equilibrium point. However, the main results of the paper remain valid. In a second paper (1988) I corrected the mistake and introduced a new kind of decomposition which can be used to simplify the analysis of extensive game models.

In the academic year 1987 8, I organized the research project 'Game Theory in the Behavioral Sciences' at the Centre for Interdisciplinary Studies at the University of Bielefeld. Among the participants who stayed there for extended periods up to one year were economists, biologists, mathematicians, political scientists, psychologists and a philosopher. The results of the project are collected in four volumes, *Game Equilibrium Models* (1991a).

The research year gave me the opportunity to pursue a number of co-operative efforts, among them a collaboration with Avi Shmida, a botanist at the Hebrew University of Jerusalem. We were interested in the question why flowers offer resources to pollinators, a problem which is deeper than one might think at first glance, and contributed a paper (1991) on the subject to volume I of *Game Equilibrium Models* which is devoted to evolution and game dynamics.

Recently Peter Hammerstein and I wrote a survey paper 'Evolutionary game theory' to be published in the *Handbook of Game Theory* (1992). We found ourselves forced to close some gaps in the literature. We were especially worried about the connection of evolutionary game theory and population genetics. The dynamics of population genetics on the basis of a fixed game pool does not necessarily work in the direction of fitness maximization. In order to justify the principle of fitness maximization one has to look at stability against mutations. For a standard model we proved that a monomorphic population state (i.e. a population state in which all individuals play the same pure or mixed strategy) is stable in this sense if and only if the population strategy is an ESS.

Evolutionary game theory is not normative but descriptive. Equilibrium is thought of as the result of a dynamic process. There is now a growing understanding among economists that in view of the limited rationality of economic agents a similar approach to equilibrium in economic game models should be taken. This makes it interesting to look at game learning processes. For a long time I was worried about the notorious instability of equilibrium points in mixed strategies with respect to learning processes. After much thinking about this problem, I discovered that an element of anticipation can be a stabilizing influence. I developed a theory of anticipatory learning in two-person games, published in volume I of *Game Equilibrium Models* (1991b).

18. Bounds of rationality

The problem of bounded rationality has occupied my mind much more than one would think if one looks at the moderate success of my efforts in this direction. One of the few basic insights I gained over the years is the necessity to distinguish between at least two different kinds of bounds of rationality, cognitive bounds and motivational bounds.

Cognitive bounds arise from the limits of the human capability to think and to compute. However, in some cases rationality is limited in a different way. This became clear to me when I discovered the chain store paradox (1978). This game has a clear backward induction solution which nevertheless is unacceptable even if all players are game theorists with common knowledge about this fact. I came to the conclusion that here, as in the finitely repeated prisoner's dilemma, we lack behavioural trust into abstract induction arguments.

I proposed a three-level theory of decision-making, in which decisions arise on up to three levels: the routine level, the level of imagination and the level of analysis. Routine decisions arise spontaneously without any thinking. The level of imagination derives decisions from selected scenarios, which are vividly imagined courses of future play of limited length. Backward induction arguments, which span more than a few periods, require the abstract thinking performed on the level of analysis. If possible decisions arise on several levels, then a final decision has to select which of them becomes effective. This final decision is a spontaneous routine decision.

When I submitted this paper to a leading journal, the referee asked me to eliminate the three-level theory which he considered to be irrelevant. Since I thought that this part of my paper was very important I was not willing to do this and eventually, after several years of delay, I published the paper elsewhere.

The attempts to solve the chain store paradox in the framework of rational game theory (Kreps and Wilson, 1982a) evade the issue by addressing a modified game with elements of incomplete information instead of the original chain store game. A related backward induction paradox appears in the finitely repeated prisoner's dilemma, where one experimentally observes co-operation followed by an end-effect (Selten and Stoecker, 1986). The incomplete information explanation (Kreps *et al.*, 1982) of this phenomenon is inadequate, since the typical pattern is learnt only after some experience and not established at once by rational analysis. Stoecker and I have explained our experimental data by a behavioural learning model.

The backward induction paradox is due to what I call motivational bounds of rationality. It is not a lack of cognitive power but rather a failure to behave according to one's rational insights that is important here. Many phenomena of everyday life can be understood as caused by motivational bounds of rationality. Somebody who is convinced that it would be best for him to stop smoking may nevertheless find himself unable to do this.

Recently, in my paper on anticipatory learning (1991b), I developed a related and, maybe, even more radical view of the nature of motivational bounds of rationality, the decision emergency hypothesis. This hypothesis maintains that conscious thinking does not have the task of making decisions. The rational mind is like an adviser to a king. The king is a subconscious hidden mechanism who makes the final decision. The king may or may not listen to the adviser. The adviser does not necessarily have to propose a decision. In some cases he may restrict himself to pointing out advantages and disadvantages of various alternatives. Decisions are not made, they emerge.

19. The strategy method

My greatest hope for progress towards an empirically based theory of bounded rationality is the strategy method: after having gained sufficient experience with a game situation, subjects are asked to write strategies in the form of computer programs. The strategies are matched in tournaments and the subjects can change their programs in the light of the result. This process is repeated several times.

I first described the strategy method in a paper (1967c) which presents an application to my experimental oligopoly game with demand inertia. At that time computer technology was not yet as developed as it is now. The participants wrote their strategies in flow-chart form and did this only once. A tournament of everybody against everybody was not within the bounds of our computing possibilities and therefore we had to be content with runs against one reference strategy written by ourselves.

The evaluation of the strategies provides valuable insights on the structure of boundedly rational behaviour. Of course, the planned behaviour produced by the strategies may be different from the spontaneous behaviour in experimental games, but one can expect that similar principles will be at work. Moreover both spontaneous and planned strategic behaviour are important in real life.

In a study by Mitzkewitz, Uhlich and myself (Selten, Mitzkewitz and Uhlich, 1988), the strategy method was applied to a 20-period supergame of a numerically specified asymmetric Cournot duopoly game. The final strategies after three rounds of programming exhibited a typical structure which suggests a new duopoly theory. Usually oligopoly theories, including game-theoretical approaches, assume optimization against definite expectations about the opponents' behaviour. Contrary to this, it is typical for the final strategies that no expectations are formed and nothing is optimized.

The subjects know by their game-playing experience that they must co-operate in order to earn higher profits. This leads to a typical approach to the problem of constructing a strategy. At first the question is asked where co-operation should take place. The answer is what we call an 'ideal point', a quantity pair derived by one of various fairness criteria, such as equal additional profits above Cournot profits or profits proportional to Cournot profits.

Now the question arises as how to induce the opponent to co-operate at one's own ideal point. The answer is what we call a 'measure-for-measure strategy', which responds to a move of the opponent nearer to one's own ideal point or further away from it by a similar move in the same direction. Thereby one hopes to induce the opponent to co-operate on one's own terms.

Typically, a measure-for-measure strategy is not used at the beginning and end of the game. In a short initial phase co-operativeness is signalled by a descending sequence of history-independent quantities. Near the end, co-operation breaks down.

Interestingly, a strategy was more successful in the final tournament, the more typical it was. This could be shown by a highly significant rank correlation of success with a measure of typicality based on 13 strategy characteristics.

The use of a measure-for-measure strategy in order to achieve co-operation at an ideal point is a reasonable approach to the finitely repeated Cournot duopoly. However, the procedural rationality of this approach is very different from the rationality of Bayesian game theory.

20. From obscurity to the mainstream

In the past I have often worked in obscure areas far away from the mainstream. This is partly due to a spirit of opposition which makes me favourably inclined towards radical departures from commonly held views and partly to an inability to be quick enough to compete successfully on the hot topics of the day. Since I am slow, I have to try to be early. However, it has happened to me several times that an esoteric specialty which attracted my attention later became a flourishing area of research. The number of game theorists was very small when I began to do work in this field. Research was concentrated on co-operative theory. It was the ruling opinion that co-operation would take place wherever it is profitable. In this situation I began to turn my attention to non-co-operative game theory.

During my stay at Berkeley in the academic year 1967 8, I gave a talk at Stanford on the demand inertia oligopoly paper (1965) in which I had introduced subgame perfectness. When people were leaving the room, I heard a young economist say to somebody else that 'this is only non-co-operative theory', a remark which captures the mood of the time.

Nowadays, non-co-operative game theory is used almost everywhere in economic theory. Game theory is no longer a specialty but rather a basic tool of economic theory.

When I began to do experimental work, experiments in economics were almost unheard of. For a long time laboratory research on economic behaviour was not taken seriously by most economists. However, the last decade has seen a tremendous rise of interest in experimental economics. The field has become respectable and it may even become fashionable.

In the many years since I became convinced of the necessity to do work on bounded rationality, I have had countless discussions with economists who firmly believe in utility maximization as an explanation of decision behaviour. The accumulating experimental evidence has made it easier and easier for me to make my point. Defendants of the orthodox view have become forced to take various positions of retreat, such as the idea that evolutionary forces must have produced an approximation to full rationality. In a fictitious dialogue on evolution, learning and economic behaviour (1990), I explained why I feel that attempts to save the rationalistic view of economic man by minor modifications have no chance of succeeding.

The interest in bounded rationality is increasing. We can hope for the emergence of a body of experimentally based descriptive theory. It is possible that the rationalistic approach has already reached its peak and that attention is going to shift to more realistic explanations of behaviour. It is tempting to speculate about the consequences of a trend in this direction. I expect that game models will continue to be very important in economics, but applied game theory will have to change its character. Rational game theory will remain of great philosophical significance, but a new kind of descriptive game theory will have to be developed for economic application.

Of course, my expectations for the future may be completely mistaken. The fact that I have been right before does not mean that I must be right again. However, I would not be surprised if, after some time, empirically based theorizing about boundedly rational behaviour becomes a substantial part of mainstream economics.

References

Abelson, R. P. and Rosenberg, M. J. (1958) Symbolic psycho-logic: a model of attitudinal cognition. *Behavioral Science*, **3**, 1–13.

Albers, W. (1975) Zwei Lösungskonzepte für kooperative Mehrpersonenspiele, die auf Anspruchsniveaus der Spieler basieren, *OR-Verfahren*, XXI, Meisenheim, 1–13.

Avenhaus, R., Güth, W. and Huber, K. (1991) Implications of the defense efficiency hypothesis for the choice of military force structures. In R. Selten (ed) *Game Equilibrium Models*, Vol. IV. Berlin: Springer-Verlag, 256–318.

Axelrod, R. (1976) *Structure of Decision, The Cognitive Maps of Political Elites*. Princeton, NJ: Princeton University Press.

Burger, E. (1958) *Einführung in die Theorie der Spiele*. Berlin: de Gruyter.

Burger, E. (1963) *Introduction to the Theory of Games*. Hemel Hempstead: Prentice Hall.

van Damme, D. (1987) *Stability and Perfection of Nash Equilibria*. Berlin: Springer-Verlag.

van Damme, E. and Güth, W. (1991) Equilibrium selection in the Spence signaling game. In R. Selten (ed), *Game Equilibrium Models*, Vol. II. Berlin: Springer-Verlag, 283 88.

Güth, W. (1991) The stability of the Western defense alliance – a game theoretic analysis. In R. Selten (ed), *Game Equilibrium Models*, Vol. IV. Berlin: Springer-Verlag, 229–55.

Güth, W. and Kalkofen, B. (1989) *Unique Solutions for Strategic Games – Equilibrium Selection Based on Resistance Avoidance*. Lecture Notes in Economics and Mathematical Systems 328. Berlin: Springer-Verlag.

Güth, W. and Selten, R. (1991) Majority voting in the Condorcet paradox as a problem of equilibrium selection. In R. Selten (ed), *Game Equilibrium Models*, Vol. IV. Berlin: Springer-Verlag, 7–40.

Hammerstein, P. and Selten R. (1992) Evolutionary game theory. To be published in R. Aumann and S. Hart (eds), *Handbook of Game Theory*.

Harsanyi, J. C. (1963) A simplified bargaining model for the *n*-person cooperative game. *International Economic Review*, **4**, 194–220.

Harsanyi, J. C. (1967–8) Games with incomplete information played by 'Bayesian' players. *Management Science*, **14**, 157 82, 320–4, 486–502. Part I (Nov. 1967) The basic model. Part II (Jan. 1968) Bayesian equilibrium points.

Harsanyi, J. C. (1975) The tracing procedure: A Bayesian approach to defining a solution for *n*-person non-co-operative games. *International Journal of Games Theory*, **4**, 61 94.

Harsanyi, J. C. and Selten, R. (1972) A generalized Nash solution for two-person bargaining games with incomplete information. *Management Science*, **18**(5), P80–P106.

Harsanyi, J. C. and Selten, R. (1988) *A General Theory of Equilibrium Selection in Games*. Cambridge, Mass.: MIT Press.

Häselbarth, V. (1967) Literaturhinweise zur experimentellen Wirtschaftsforschung. In H. Sauermann (ed), *Beiträge zur experimentellen Wirtschaftsforschung*, Vol. 1. Tübingen: J. C. B. Mohr (Siebeck), 267–73.

Hoggatt, A., Selten, R., Crochett, D., Gill, S. and Moore, J. (1978) Bargaining experiments with incomplete information. In H. Sauermann (ed), *Beiträge zur experimentellen Wirtschaftsforschung*, Vol. 7. Tübingen: J. C. B. Mohr (Siebeck), 127–78.

Kahneman, D. and Tversky, A. (1984) Prospect theory: an analysis of decision under risk. *Econometrica*, **47**, 263–91.

Kalish, G., Milnor, J. W., Nash, J. and Nering, E. D. (1954) Some experimental *n*-person games. In R. M. Thrall, C. H. Coombs and R. L. Davis (eds), *Decision Processes*. New York/London, 301–27.

Keser, C. (1992) Experimental duopoly markets with demand inertia. Unpublished PhD dissertation, Bonn.

Kreps, D., Milgrom, P., Roberts, J. and Wilson, R. (1982) Rational co-operation in the finitely repeated prisoner's dilemma supergame. *Journal of Economic Theory*, **27**, 245–52.

Kreps, D. M. and Wilson, R. (1982a) Reputation and imperfect information. *Journal of Economic Theory*, **27**, 253–79.

Kreps, D. M. and Wilson, R. (1982b) Sequential equilibria. *Econometrica*, **50**, 863–94.

Leopold, U. (1982) *Gleichgewichtsauswahl in einem Verhandlungsspiel mit Opportunitätskosten*. Bielefeld: Pfeffersche Buchhandlung.

Leopold, U. (1985) Equilibrium selection in a bargaining problem with transaction costs. *International Journal of Game Theory*, **14**, 151–72.

Maynard Smith, J. and Price, G. R. (1973) The logic of animal conflict. *Nature*, **246**, 15–18.

Marschak, Th. and Selten, R. (1974) *General Equilibrium with Price Making Firms*. Berlin: Springer-Verlag.

Marschak, Th. and Selten, R. (1977) Oligopolistic economies as games of limited information. *Zeitschrift für die gesamte Staatswissenschaft*, **133**, 385–410.

Marschak, Th. and Selten, R. (1978) Restabilizing responses, inertia supergames and oligopolistic equilibria. *Quarterly Journal of Economics*, **92**, 71–93.

von Neumann, J. and Morgenstern, O. (1944) *Theory of Games and Economic Behavior*. Princeton, NJ: Princeton University Press.

Potters, J., van Winden, F. and Mitzkewitz, M. (1991) Does concession always prevent pressure? In R. Selten (ed), *Game Equilibrium Models IV: social and political interaction*. Berlin: Springer-Verlag, 41–63.

Pruitt, P. G. (1970) Motivational processes in the decomposed prisoner's dilemma game. *Journal of Personality and Social Psychology*, **14**, 227–38.

Ricciardi, F. M. (1957) *Top Management Decision Simulation, The AMA Approach*. New York.

Sauermann, H. (ed) (1967, 1970, 1972, 1978) *Beiträge zur experimentellen Wirtschaftsforschung*, Vols 1, 2, 3 and 7, respectively. Tübingen: J. C. B. Mohr (Siebeck).

Sauermann, H. and Selten, R. (1959) Ein Oligopolexperiment. *Zeitschrift für die gesamte Staatswissenschaft*, **115**, 427–71.

Sauermann, H. and Selten, R. (1962) Anspruchsanpassungstheorie der Unternehmung. *Zeitschrift für die gesamte Staatswissenschaft*, **118**, 577–97.

Selten, R. (1960) Bewertung strategischer Spiele. *Zeitschrift für die gesamte Staatswissenschaft*, **116**, 221–81.

Selten, R. (1964) Valuation of n-person games. In *Advances in Game Theory*, Annals of Mathematics Studies no. 52, Princeton, NJ: Princeton University Press, 565–78.

Selten, R. (1965) Spieltheoretische Behandlung eines Oligopolmodells mit Nachfrageträgheit. *Zeitschrift für die gesamte Staatswissenschaft*, **121**, 301–24, 667–89.

Selten, R. (1967a) Investitionsverhalten im Oligopolexperiment. In H. Sauermann (ed), *Beiträge zur experimentellen Wirtschaftsforschung*, Vol. 1. Tübingen: J. C. B. Mohr (Siebeck), 60–102.

Selten, R. (1967b) Ein Oligopolexperiment mit Preisvariation und Investition. In H. Sauermann (ed), *Beiträge zur experimentellen Wirtschaftsforschung*, Vol. 1. Tübingen: J. C. B. Mohr (Siebeck), 103–35.

Selten, R. (1967c) Die Strategiemethode zur Erforschung des eingeschränkt rationalen Verhaltens im Rahmen eines Oligopolexperiments. In H. Sauermann (ed), *Beiträge zur experimentellen Wirtschaftsforschung*, Vol. 1. Tübingen: J. C. B. Mohr (Siebeck), 136–68.

Selten, R. (1970a) *Preispolitik der Mehrproduktenunternehmung in der statischen Theorie*. Berlin: Springer-Verlag.

Selten, R. (1970b) Ein Marktexperiment. In H. Sauermann (ed), *Beiträge zur experimentellen Wirtschaftsforschung*, Vol. 2. Tübingen: J. C. B. Mohr (Siebeck), 33–98.

Selten, R. (1972) Equal share analysis of characteristic function experiments. In H. Sauermann (ed), *Beiträge zur experimentellen Wirtschaftsforschung*, Vol. 3. Tübingen: J. C. B. Mohr (Siebeck), 130–65.

Selten, R. (1973) A simple model of imperfect competition where 4 are few and 6 are many. *International Journal of Game Theory*, **2**(3), 141 201. (Reprinted in Selten, R. 1988: *Models of Strategic Rationality*, Dordrecht: Kluwer.)

Selten, R. (1975a) Reexamination of the perfectness concept for equilibrium points in extensive games, *International Journal of Game Theory*, **2**(3), 25–55. (Reprinted in Selten, R. 1988: *Models of Strategic Rationality*, Dordrecht: Kluwer.)

Selten, R. (1975b) Bargaining under incomplete information - a numerical example. In O. Becker and R. Richter (eds), *Dynamische Wirtschaftsanalyse*. Tübingen: J. C. B. Mohr (Siebeck), 203–32. (Reprinted in Selten, R. 1988: *Models of Strategic Rationality*, Dordrecht: Kluwer.)

Selten, R. (1977) The scenario bundle method. In *Research Conference on Strategic Decision Analysis Focusing on the Persian Gulf*, 5–56.

Selten, R. (1978) The chain store paradox. *Theory and Decision*, **9**, 127–59. (Reprinted in Selten, R. 1988: *Models of Strategic Rationality*, Dordrecht: Kluwer.)

Selten, R. (1980) A note on evolutionarily stable strategies in asymmetric animal contests. *Journal of Theoretical Biology*, **83**, 147 61. (Reprinted in Selten, R. 1988: *Models of Strategic Rationality*, Dordrecht: Kluwer.)

Selten, R. (1981) A non-co-operative model of characteristic function bargaining. In V. Böhm and H. Nachtkamp (eds), *Essays in Game Theory and Mathematical Economics in Honor of Oskar Morgenstern*. Wissenschaftsverlag Bibliographisches Institut, 131 -51. (Reprinted in Selten, R. 1988: *Models of Strategic Rationality*, Dordrecht: Kluwer.)

Selten, R. (1983a) A model of oligopolistic size structure and profitability. *European Economic Review*, **22**, 33–57. (Reprinted in Selten, R. 1988: *Models of Strategic Rationality*, Dordrecht: Kluwer.)

Selten, R. (1983b) Evolutionary stability in extensive two-person games. *Mathematical Social Sciences*, **5**, 269–363.

Selten, R. (1983c) Equal division payoff bounds for three-person characteristic function experiments. In R. Tietz (ed), *Aspiration Levels in Bargaining and Economic Decision Making*, Springer Lecture Notes in Economics and Mathematical Systems 213, 265 75. (Reprinted in Selten, R. 1988: *Models of Strategic Rationality*, Dordrecht: Kluwer.)

Selten, R. (1984) Are cartel laws bad for business? In H. Hauptmann, W. Krelle and K. C. Mosler (eds), *Operations Research and Economic Theory*. Berlin: Springer-Verlag, 86 117. (Reprinted in Selten, R. 1988: *Models of Strategic Rationality*, Dordrecht: Kluwer.)

Selten, R. (1987) Equity and coalition bargaining in experimental three-person games. In A. Roth (ed), *Laboratory Experimentation in Economics – Six Points of View*. Cambridge: Cambridge University Press, 42 98.

Selten, R. (1988) Evolutionary stability in extensive two-person games, correction and further development. *Mathematical Social Sciences* **16**(3), 223–66.

Selten, R. (1990) Evolution, learning, and economic behavior. *Games and Economic Behavior*, **3**, 3–24.

Selten, R. (ed) (1991a) *Game Equilibrium Models*, Vols I IV. Berlin: Springer-Verlag.

Selten, R. (1991b) Anticipatory learning in two-person games. In R. Selten (ed), *Game Equilibrium Models*, Vol. I. Berlin: Springer-Verlag, 98–154.

Selten, R. (1991c) Balance of power in a parlor game. In R. Selten (ed), *Game Equilibrium Models*, Vol. IV. Berlin: Springer-Verlag, 150–209.

Selten, R. (1992) A demand commitment model of coalition bargaining. In R. Selten (ed), *Rational Interaction, Essays in Honor of John C. Harsanyi*. Berlin: Springer-Verlag, 245-82.

Selten, R. and Berg, Claus C. (1970) Drei experimentelle Oligopolspielserien mit kontinuierlichem Zeitablauf. In H. Sauermann (ed), *Beiträge zur experimentellen Wirtschaftsforschung*, Vol. 2. Tübingen: J. C. B. Mohr (Siebeck), 162–221.

Selten, R. and Güth, W. (1982) Equilibrium point selection in a class of market entry games. In *Games, Economics, and Time Series Analysis*. Wurzburg: Physica-Verlag, 101–16.

Selten, R. and Güth, W. (1991) Original and fake - a bargaining game with incomplete information. In R. Selten (ed), *Game Equilibrium Models*, Vol. III. Berlin: Springer-Verlag, 186–229.

Selten, R. and Leopold U. (1983) Equilibrium point selection in a bargaining game with opportunity costs. *Economie Appliquée*, **XXXVI**(4), 611–48.

Selten, R., Mitzkewitz, M. and Uhlich, G. (1988) Duopoly strategies programmed by experienced players. Discussion Paper, Sonderforschungsbereich 303, No. B-106, University of Bonn.

Selten, R. and Schuster, Klaus G. (1968) Psychological variables and coalition forming behavior. In K. Borch and I. Mussin (eds), *Risk and Uncertainty*, London/Melbourne/Toronto/New York: St Martin's Press, 221–46.

Selten, R. and Shmida, A. (1991) Pollinator foraging and flower competition in a game equilibrium model. In R. Selten (ed), *Game Equilibrium Models*, Vol. I. Berlin: Springer-Verlag, 195–256.

Selten, R. and Stoecker, R. (1986) End behavior in sequences of finite prisoner's dilemma supergames. *Journal of Economic Behavior and Organization*, **I**, 47–70.

Selten, R. and Tietz, R. (1972) Security equilibria. In R. Rosecrance (ed), *The Future of the International Strategic System*, San Francisco Scranton/London/Toronto: Chandler, 103–22.

Simon, H. A. (1957) *Models of Man*. New York: Wiley.

Uhlich, G. R. (1990) *Descriptive Theories of Bargaining*. Lecture Notes in Economics and Mathematical Systems, No. 341. Berlin: Springer-Verlag.

6

Joseph E. Stiglitz

Reflections on economics and on being and becoming an economist

I had misgivings about being asked by Professor Heertje to write this piece: I was flattered that he thought that others might be interested in my ruminations on economics, and on being and becoming an economist; but such invitations are usually reserved for the greybeards of our profession, and though a quick glance in a mirror confirmed that I was *becoming* a greybeard, I hardly felt like one, and, more importantly, I did not want to think of myself as ever becoming one. Rather, I thought of myself as 'an old young turk' – fighting the establishment even as I was becoming part of it; trying to forge new ground, even as I was trying to consolidate what progress had already been made, and even as I remained sceptical of some of the alleged advances of the still younger turks that surrounded me. Upon reflection, it seemed perhaps a good vantage point for some reminiscences, different from that usually taken at the end of a career, looking over the full landscape of one's development within the context of the development of the profession over a half century.

I will not try to present a coherent narrative, nor a clear and concise statement of my *Weltanschauung*. I hope, however, that by the end of these reminiscences, certain aspects of my views about economics, and why I hold these views, will become apparent.

The paper is divided into five parts. In the first, I discuss certain aspects of my upbringing and education; in the second, I turn to my early career; in the third, I turn to my later career, focusing largely on issues of information economics, which have occupied much of my attention during the past quarter of a century; in the fourth, I discuss briefly two other strands of research; and in the fifth, I present briefly some views on the current state of the profession and future directions of research.

1. Early beginnings and my introduction to economics

I will pass over quickly the early awakenings of my interest in economics, not because they might not be important, but because of my scepticism of the value or credence of any interpretation of these early events. Was it an accident that I was born, and spent the first 17 years of my life, in the same relatively small (population, at the time, around 150,000) industrial town, Gary, Indiana, that gave rise to Paul Samuelson, as well as several other well-known economists?

My father, a fiercely independent man who ran his own small insurance agency, had strong views on economics, a blend of conservatism – a suspicion of big government (and big business) – combined with what we might recognize today as a modern adaptation of the Jeffersonian belief in the value of the small farmer, with its stress on the values of individual self-reliance and independence. These views were combined with a recognition that there were social and economic ills in our society about which 'something ought to be done'. Politically, my parents were in the centre: both of my parents were strong supporters of FDR, though I suspect my father may have voted for Eisenhower, but not Nixon. Whether they voted for Reagan or not, I do not know, but if they did, I am sure they came to have misgivings.

As I grew up, and flirted with leftist ideas, we had many discussions - though I perhaps might better describe them as arguments; I am sure neither of us convinced the other, though in retrospect, I suspect my father's arguments were probably more correct than mine.

I suspect my uncle also had some influence: he was a wealthy liberal, a strong advocate of government social programmes, and above all, of civil rights. He saw conservative leanings in me, and was greatly concerned. Though he was a strong democrat, he was highly critical of Kennedy's moves against the Teamsters, and Jimmy Hoffa, worried both about their effects on the labour movement, and their implications for the principles of civil rights.

For 11 years, from second grade through to twelfth, I went to the same public school, Horace Mann school, named after a famous American educator who was instrumental in the development of the American public education system. Gary had been founded by the US Steel Company in 1906 as a planned company town. Its schools were innovative: they attempted to integrate into America the new immigrants that came to work in the steel mill, to provide them with a balance of academic and practical-life education. I received training not only in the standard academic subjects, but also in printing and electrical skills. The spirit of innovation which had marked their earlier days was, unfortunately, waning, but there remained a number of dedicated teachers; they showed the flexibility of allowing me to proceed at my own pace: almost the only formal maths class I had, after seventh grade, was advanced calculus, after I entered college.

I loved both maths and social studies. I naturally became involved in the high-school debating team. Though I have misgivings about the sophistry which it might have promoted, I learned not only about the ability to see both sides of an argument, but also an enormous amount about economics and politics. One year, we debated changes in the American farm programme; the institutional details I learned then have served me in good stead since – though it was not until many years later that I fully appreciated the economic issues.

A third interest developed in those years: writing and editing. I became the national editor of the magazine published by the National Forensic Society, the high-school debating society.

Amherst College

At the age of 17, I left home to attend Amherst College, a small liberal arts college (at the time, about 1000 men) in western Massachusetts. My brother had, upon the advice of his high-school counsellor, gone there, and enjoyed it immensely. It was a big move: I had never been East, and my parents were sceptical of the East – they were not sure that the values which they held, including their religious faith, would find support in an elitist establishment college, though they were proud that I was accepted and had won a scholarship.

Amherst provided me with a first-rate liberal arts education, and for that I shall always be indebted. My teachers stressed the importance of asking the right question, the importance of scepticism, of questioning. And while no one can teach creativity or the creative process, they did as good a job as one could in giving all the prerequisites, both in providing tools, and, more importantly, of creating the appropriate disposition: they stressed the willing suspension of disbelief – one could not solve all problems at one time, and one had to proceed with some working hypotheses; they stressed the role of intuition as well as rigour. They often taught by use of the Socratic method: a series of questions and answers as we seemed to explore a topic together, rather than a lecture, with the kind of spoon-feeding of received wisdom to passive listeners that often passes for 'good teaching'. They emphasized how problems had to be looked at from different perspectives. What and how they taught often seems a far cry from the way economics is so often approached even in good universities.

I took but three semester courses (and a reading course) in economics at Amherst, but each had great influence on me. My principles course (we did all of Samuelson in one semester) was taught by Jim Nelson, a dynamic transportation economist with greater concern for economic policy. How alive and *important* he made economics seem! This was followed by a semester of micro-economics and a semester of macro-economics taught by Arnold Collery, who later went on to become a distinguished Dean of Columbia College. The micro-economics course illustrated the exciting way that economics was taught. We had no text: we read Abba Lerner's *Economics of Control*, and used it as a vehicle for understanding the basic micro-economic principles. My interest in comparative economic systems (and market socialism in particular) traces back to this beginning course. Lerner's book was analytical, but not mathematical, and one of our tasks was to translate his book into simple mathematics.

The one fault that I could find with my macro-economics course was that it made macro-economics seem like a finished subject. The whole model seemed well worked out. What was there for us – who had arrived too late on the economics scene to do? From our current perspective, there is a certain irony in this.

MIT

At the end of my junior year at Amherst, I decided to go on in economics. Up to that point, I had taken a mix of courses, almost equally divided between physics, English, economics, history and mathematics with a touch of philosophy, biology, chemistry, etc – a true liberal arts course (without the courses in ceramics and Zen Buddhism that seem to constitute a standard part of today's liberal arts courses). I loved them all: the excitement of new ideas, the opportunity to stretch one's mind, to come into contact with the greatest thinkers of all ages. I was a child of the 1960s, able to pass through these years with hardly any attention to what I wanted to do when I grew up. I had spent one summer working as a research assistant at Woods Hole Oceanographic Institute, which included an expedition out to sea. This was exciting and wonderful, and one of my good friends stayed on to become an oceanographer.

But I finally decided that I wanted to become more 'socially involved' than physics would allow me; and I loved the blend of history, social studies and mathematics which it seemed economics might be able to provide. I could not fully shake off the indoctrination of my parents, of the importance of providing service to others meant, I believe, to motivate me to become a doctor, or perhaps a lawyer. I became an economist, *wanting* to make a difference how, I had only the vaguest ideas and dreams, but pictures of the impoverished in America and the destitute in the Third World certainly passed through my mind.

What an exciting place MIT was at that time, with scholars such as Bob Solow, Paul Samuelson and Franco Modigliani, and visitors such as Frank Hahn and Ken Arrow! There were lesser eminences, most of whom we treated with almost equal awe and respect. There were the up-and-coming terrors, like Franklin Fisher, being groomed to replace the great eminences in a decade or two. To be sure, there were the lightweights (often distinguished in their field), but who would have had a hard time passing the MIT comprehensives.

Growth theory was the fad of the time – a fad which swept economics like no fad since Keynes, and without the scepticism which had greeted that work. And MIT was the centre of this research world. There was another centre in Cambridge, UK, in which certain heresies were occurring, but with little impact on this side of the Atlantic.

Within the USA, there were at that time but two other places worth mentioning. Chicago had great economists, such as Milton Friedman, who had the unfortunate habit of taking seriously the competitive equilibrium model which we were taught at MIT. But when we were taught it, it was explained to be an unrealistic benchmark. No one actually believed that all markets cleared, that there was no role for government intervention, or even that the government interventions should be limited to correcting externalities and providing public goods. We were never told what was wrong with our models, only that they were not applicable, and the task of creating the 'right model' which would tell us what to do did not seem high on the MIT research agenda. Thus, we laughed at Milton Friedman, but I sometimes think the last laugh was on ourselves. For we felt, I think, rather divided. This bothered several of the graduate students. George Akerlof and I had frequent conversations trying to get at what was wrong with the models we were being taught: some of the problems, like uncertainty, a lack of a complete set of futures markets, and the absence of endogenous technical change seemed researchable topics, and we and others immediately began work on those areas. Others seemed more important, but more difficult. We were concerned about the informational assumptions which went into the model. I recall conversations which provided the germ of models on selection (I believe the metaphor George used was that of an egg sorter), on search (frictional unemployment was modelled using a bathtub theorem), and on statistical discrimination. In the rush to finish the coursework and to complete a thesis, these ideas had to wait several years to be developed.

Not only was there a concern that the models we employed left something important out, there was a concern that attention was not being focused on the most important questions. Those of us who had entered economics with a touch of the social reformer in us wanted to do something about the wrongs of the world. What we were taught was that the economy was Pareto-efficient. There were a few members of the Faculty, usually on the periphery, who were concerned with problems of income distribution, but in the intellectual hierarchy which was so plain at MIT, these did not count for much.

The period was, however, the golden age of development economics, and two of the members of our circle included Louis Lefebre and Steve Marglin, both of whom were bright young men doing work in development planning. Marglin had been a Harvard Junior Fellow, which gave him a certain panache; his sometimes abrasive style confirmed his potential to be a great man. I never got intellectually attracted to the problems with which they were concerned: somehow, I was suspicious that solving a linear programme problem would transform India into a modern state. Shortly afterwards, George went off to spend a year in India, and I spent two long summers at the University of Nairobi, but that is getting ahead of the story.

I must have entered MIT in a fairly over-confident mood. I believed that with three semester courses at Amherst, graduate work at MIT would be a breeze. I signed up for six courses, and finished my coursework in a year and a half. The first year was fun, too, because it represented my one experience with poverty. When I applied, and got admitted, to MIT late in the spring, they could only come up with $1000 of fellowship money. After paying for my room, that meant I had a dollar a day for food. (The food stamp programme had not yet started.) Though I knew the situation was only temporary, it made me keenly aware of what life must be like for millions and billions in the world who can hardly eke out a living.

After finishing my first year's coursework, I got a summer job that I have had a hard time living down since: I was hired to edit (what became the first two volumes of) Samuelson's *Collected Papers*. For many years, I was best known and frequently introduced (particularly outside the USA) as Samuelson's editor: I wanted, of course, to be recognized for my own work. My task was to read all of Samuelson's work to that date, to look for errors (if any) and to organize the papers in a coherent manner. From my perspective, it was an excellent way of studying for the comprehensive examinations.[1]

After finishing my coursework, I took to writing, thinking and doing research. I list the verbs in that order deliberately. I loved to write. I found that by sitting down to write something, I forced myself to think in a structured way. And, having forced myself to think in a structured way, certain research questions were naturally posed, and had to be resolved, for the writing to proceed.

My choice of topics during this period reflects both my interests and the influences of those around me. I wrote two unsuccessful papers on Marxism, one trying to explain the colonial period, the other trying to reconcile Marx's predictions concerning the decline of capitalism.

Other papers were concerned with capital theory. One was on the rate at which wine prices increase over time – testing the Austrian capital model, a project which I had undertaken under the direction of Will Baumol and Dick Quandt at Princeton, where I spent the summer prior to coming to MIT.

But most of my energies were devoted to MIT's research programme, understanding growth models. I developed a method of estimating vintage capital goods models with embodied technological change. It was a massive study, marred by my lack of command of the appropriate ways to introduce lags in an acceptable way. I sometimes dream of coming back to this topic. This econometric work was not my love.

I look back with fondness at the two years I spent at MIT as a graduate student. Boston was a big city – the first big city I had ever lived in. The Faculty took the graduate programme seriously. There were first-rate lectures: no one could beat Solow in presenting a lecture which was both entertaining and which went to the heart of the matter; for me, Samuelson's lectures were great, an opportunity to see a brilliant mind in action, though many of my classmates, I am afraid, would have preferred a more mundane, more structured course.

These differences in reactions drew my attention forcefully to a distinction of which I have been aware ever since: how different minds work, and how different students need to be taught, and how different the pleasures which different students get out of their graduate studies. For some, the first flush of ideas is what triggers the smile, and the light in their eyes flickers with excitement. For others, academia is like work: not until they have mastered the subject can they sit back, relax and, enjoying the sense of accomplishment, proceed to think of the next steps. Psychologists talk of linear and non-linear thinkers: I have had many students of both types.

But it has often been said that what made MIT a truly great place was its graduate students, and I agree. The list of students, now famous economists, coming or going within the span of time that I was there was enormous: Bob Hall, Bob Gordon, George Akerlof, Bill Nordhous, Eytan Sheshinki, Myrdal Datta Chaudhuri (now head of the Delhi School of Economics)...[2]

When all is said and done, MIT must have been doing something right. To be sure, it had good inputs. But it succeeded in enabling those it took to reach their potential (some even to exceed it). I have often pondered what it was that made it so successful. Surely one aspect was how it blended applied economics with theoretical economics: mathematical economics was not an abstruse subject, remote from the central economic questions of the day. It also blended economic intuition with mathematical rigour – I would say, just the right amount of rigour, but my views on this were undoubtedly shaped by my MIT education. (I shall return to this theme later.) Finally, the methodology of using simple formal models to analyse the essence of a particular problem – of which Solow's 1956 paper is the example *par excellence* – has proved to be enormously powerful; because it is so commonplace today, we take it for granted, but at the time, it was an approach that was relatively rare in economics.

Cambridge

After two years at MIT, I won a Fulbright Fellowship to Cambridge. My arrival in Cambridge had caused quite a stir. Joan Robinson wanted me to go back and do the undergraduate exam (called the Tripos). She was convinced that I had learned less than nothing in two years of the MIT graduate programme. David Champernowne, I am told, saved the day, and got me enrolled as a graduate student, with Joan as my supervisor. That was my wish, of course: I had come to Cambridge hoping to hear the other side of the story, as the expression goes, 'from the horse's mouth'. And I did. As is the custom in Cambridge. I met with my supervisor once a week. She would assign essays the week before, which I would turn in shortly before our meeting. Our typical meeting would proceed as follows: she would begin by yelling at me, about one answer or another, leading up to the question, 'Are you serious about economics, for if you are not, get out! I have enough of this guff from Solow and Samuelson: I don't need to take it from you!' I would meekly reaffirm my interest in serious economics, and she would then attempt to set me on the true and narrow road. I suspect we egged each other on. She would ask questions like, 'What is the marginal product of capital?' and when I came back with the obvious answer (not the answer that she wanted, which was that it was not a well-defined question), she would fly off the handle. But she was totally possessed by the subject. We would bump into each other at the grocery store or bicycling to Grantchester, and a conversation we had begun at coffee the previous day would be picked up, as if there had been no interruption. We would pass notes to each other, sometimes through the mail - and in those days, one could get in two full rounds in a single day. There was a high level of activity. We must have been doing something important! We must have been on to something!

Upon reflection, maybe it was just a tempest in a teapot. Yet, such tempests, whether real or just in a teapot, not only give verve to life, they often lead to good research. They often lead to lousy research as well, though whether the appearance of lousy research in such an area is the result of 'trade creation or trade diversion' I am not sure.

After six months of being supervised by Joan, I switched to Frank Hahn. The list of his students and former students gathered around him in Cambridge at that time reads like a *Who's Who* of British economics Jim Mirrlees, Christopher Bliss, Partha Dasgupta, Tony Atkinson, David Newbery, Geoff Heal... (My first teaching experience was tutoring at Cambridge, and several of these were among my first students. I am not sure that I got an unbiased introduction into the pleasures of teaching.) These became my lifelong friends. There was a spirit about Cambridge at the time, and this group in particular, which was distinctive. It took economics seriously. The task of economics was to understand how capitalism works *and* to try to use economics to make a better world to reduce inequality and to help the Third World countries develop. It believed that such an understanding could make a difference.

The spectrum of views about capitalism was far broader, and centred more to the left, than those to which I had been exposed at MIT. Joan was awaiting the immediate demise of capitalism, and she looked with admiration to what was going on in China. She saw the Cultural Revolution as a great victory: I never had a chance to get her reactions to the events in China as they unfolded in the 1970s and early 1980s.

Frank Hahn was perhaps at the centre of Cambridge. During the period I was there he wrote his famous paper on dynamic instability, in which he showed that a growth model with two or more capital goods, and with perfect foresight about rates of return, was unstable. This motivated one of my earlier research projects, described below. His view was not just that this was a mathematical curiosity, but that it was another 'nail in the coffin of capitalism'.

Nicky Kaldor was another Great with whom I had the opportunity to interact. He too was grappling both with what was wrong with capitalism, and what was wrong with our models of capitalism. His criticisms of the production function were no less vehement than Joan's, but they were much more thought out. One of his concerns was with what he saw as the static nature of the production function. Technical change which, to use modern terminology, he saw as endogenous was at the centre of an understanding of capitalist economies. (Issues of technical change had also been at the centre of our discussions in Chicago under Uzawa;[3] Kaldor's intuitive approach complemented nicely the more formal approach that we had taken there.) When Atkinson and I wrote our paper on 'localized learning' (1969) he expressed the view that it captured well many of his views on technical progress. He was also greatly concerned with imperfect competition and returns to scale, issues to which the profession as a whole did not turn until many years later.

While Kaldor, Hahn and Robinson had a certain flamboyancy in their approach to economics, James Meade, David Champernowne and Michael Farrell were no less influential, both in intellectual discussions within Cambridge in general, and on me in particular. All three showed that there were exciting economics problems that were not at the centre of the profession's current fad.

But while I learned from all of these grey eminences – and the younger eminences such as Bliss and Mirrlees, I had a special relationship with Frank, who was my supervisor. Frank was a hard taskmaster, but one who brought out the best in his students. I remember showing him a paper; after railing about one aspect or another of it for half an hour, he said he would take it home and take a closer look. A week later, he had accepted it to be published in the *Review of Economic Studies* of which he was an editor.[4]

That paper was concerned with the dynamics of a two-sector version of the Pasinetti growth model and must rank among my least cited papers. Another paper out of my thesis which also contends for that prize is one in which I showed the North–South flow of vintage machines in response to differences in North–South wages. Though this paper (Stiglitz, 1969a) was an early attempt to model flows of technology between the North and South (even before these terms were invented) it illustrates the general principle that a paper written too early may be condemned to obscurity no less than a paper written too late.

The third chapter of my thesis addressed the switching controversy, the question of whether or when you could say some technology was more capital-intensive than another.[5] For Joan, reswitching was the ultimate proof that aggregate capital was a meaningless and misleading concept. Levhari (1965), a fellow MIT student, had claimed that such reversals (where a technique was used at a low interest rate, and then reintroduced at a high interest rate) could not occur. He made a mathematical mistake, giving the greatest pleasure to those in the other Cambridge. I provided a set of fairly weak sufficient conditions, under which such switching could not occur. Whether for that reason or, more plausibly, because people had just become bored with the topic, the issue seemed to fade away, even before my paper was printed.

There were two parts of my thesis and one part of the paper written while I was at Cambridge which I thought were more than just student exercises, and set off research programmes that I and others have followed for the past 25 years.

Dynamics and rational expectations

The first was an outgrowth of the work I had done on the Cambridge Cambridge controversy. The real issue, I believed, had to do with the actual dynamics of the economy, not the steady states upon which the UK Cambridge economists (with the exception of Hahn) seemed to be focusing. I began by asking whether it was possible that along a dynamic path we could get *recurrence* of techniques, suggesting that growth was not necessarily *monotone*. The answer, in a simple life-cycle model that I constructed, was clearly yes, even with rational expectations (Stiglitz, 1973c).

But like many research projects, unanticipated results turned out to be more interesting than the anticipated results. Following Frank Hahn's classic 1966 paper showing the dynamic instability of capitalist economies with more than one capital good, under the assumption of perfect foresight (what would now be called non-stochastic rational expectations), a number of us at MIT had attempted to ascertain how robust the result was. Shell and I constructed a simpler model confirming Hahn's result, showing in fact that in finite time, for all except just the right initial conditions, the equal profits arbitrage equation simply could not hold (provided prices were constrained to be non-negative) (Shell and Stiglitz, 1967). Beyond that, we showed that with sufficiently inelastic adaptive expectations, the system was stable. This was among the first of a class of results which suggested that frictions (lack of perfect foresight, etc.) actually serve to stabilize the economy. Though in the shorter run, these frictions mean the economy presumably is allocating resources less efficiently, over the longer run, the economy seems to perform better, at least more of the time. This general principle, that imperfections are necessary and important for the functioning of the economy, was a theme I revisited later with Sandy Grossman, in analysing incentives to collect information (Grossman and Stiglitz, 1976, 1980a).[6] I have no metatheorem defining the conditions under which some form of sand thrown into the economy improves performance, but at least we should be alert to the possibility.[7]

While these results seemed to suggest that in the long run, the economy's growth path was determinate, and the economy exhibited considerable stability, in other work (Stiglitz, 1973b), I had shown that even with rational expectations, there were many equilibrium paths starting from any initial conditions. In other work, Sidrauski, Shell and I showed that, in an economy with money and growth, the equilibrium might be a centre, that is, given an initial capital stock, the economy might converge to equilibrium from many different values of the initial price level (Shell, Sidrauski and Stiglitz, 1969). Of course the paths of convergence appear different. This multiplicity of paths posed a particular problem for decentralized economies without futures markets: when there is a unique equilibrium path, one might (unpersuasively, in my judgement) argue that everyone could figure out where the economy *had* to go; when there were multiple equilibrium paths, this was not true. Without some form of market or government co-ordination, the story of how everyone was to know which of the possible paths the economy was to choose became critical. Economic theory provided no good answers.

There is a curious irony in the distinction between our research programme and the programme that later developed out of Chicago. We disliked dynamics which had the property that a single trajectory leads to the steady state. As we saw it, the absence of futures markets extending infinitely far into the future provided no assurance that the economy would set off down that trajectory. We did not believe that individuals had the foresight to solve the full differential equation of the dynamics of the capital goods (money). Thus, for us, obtaining a centre (a dynamic model in which all paths within a region converged to the equilibrium) seemed a real accomplishment; and obtaining a saddle-point trajectory, with only one path converging to the equilibrium was a disaster. The Chicago economists reversed the logic, because they wanted there to be a determinate equilibrium. They never explained very well how, in the absence of futures markets, anyone solved for the infinite future, and took actions to ensure that the price was set right on the convergent trajectory. It seemed a matter of faith that, somehow, this would happen.

Distribution of income and wealth

The second major paper (Stiglitz, 1969b) to come out of my thesis work was on the distribution of income and wealth among individuals. Earlier work in the Kaldor tradition had developed a theory of the distribution of income among factors of production. But what most of us cared about was, how was income to be distributed among individuals, not among factors? To answer this question, one had to have a theory that described the evolution of factor endowments over generations. I developed a simple taxonomy identifying the critical functions (the savings function, the bequest function, the reproduction function), and used this to obtain some remarkable results, e.g. convergence of all families to the same level of per capita income (a clearly unrealistic result), and an identification of the factors which inhibit this convergence (the role of non-equal inheritance, inherited differences in abilities, even with a regression towards the mean, imperfect annuity markets, etc.). One of the great pleasures of my sojourn at Cambridge occurred when James Meade took my paper, and wrote a 20-page note commenting and extending upon it. This was the kind of interaction which defines a community of scholars, so rarely seen in modern 'business' academia. The question of the determinants of the distribution of income among individuals has, unfortunately, not received the continuing attention it deserves.[8]

Disequilibrium macro-economics and dynamics

The third topic (not actually included in my thesis) was a paper which eventually became written jointly with Bob Solow (Solow and Stiglitz, 1968). We began by asking: could we formulate a model which was consistent both with the marginal productivity theory of wage determination and the Cambridge (Marx–Kaldor) theory of distribution? But to answer that question, we formulated a simple macro-economic model, of the kind that today is called a disequilibrium model. We formalized the notion that with fixed prices and wages, the short side of the market dominates. We reformulated the demand and supply functions, taking into account the fact that individuals might not be able to sell all the labour they would like (or firms might not be able to buy all the labour they would like). We were, I am afraid, a few years ahead of our time, and we made one fatal flaw: we thought it obvious that the savings and labour supply decisions could be derived from simple utility functions. But we did not bother to prove this, to set up the trivial household and firm-constrained maximization problems. Barro and Grossman (1971) did this, surely a worthwhile extension of what we had done, but hardly changing any of the economics.

At the same time, we were able to describe the short-term dynamics of adjustment (e.g. employment and wage dynamics), an improvement, I think, over much of the later literature which analysed the short-run equilibrium and left it at that. We were, I think, able to give a convincing analysis of why, even in the presence of downward flexibility of wages and prices, full employment equilibrium might not be quickly restored.

The issue of dynamics I still think lies at the heart of macro-economic analysis, and I will return to this later in this chapter.

Towards the middle of my year (1965–6) at Cambridge, I was elected to a Tapp Junior Research Fellowship. This had three marked advantages: I moved out of the 'digs' in which I had lived. I am afraid that my style of life did not comport well with the views of English old maids who tend to run the digs, at least in those days. My landlady was as relieved to see me go as I was to leave. I moved into splendid rooms in Gonville and Caius College.

Equally important, the fellows' dinners and lunches at Gonville and Caius led me to new gastronomic heights. My mother had been an excellent baker, but she overcooked vegetables in the best tradition of Midwestern cooking. English cooking has a deservedly low standing, but the colleges – at least for their fellows – provided a welcome respite.

Finally, it brought me into contact with a range of scholars, both in and out of economics. Conversation with Michael Farrell,[9] my mentor at Caius, was particularly memorable. Issues such as 'To what extent can we explain aggregate savings by the life-cycle model?', 'Can there be rational destabilizing speculation?', 'What role does evolution play in competitive theory?' were all questions discussed and debated, long before they became popular within the profession as a whole.

My year at Cambridge had been marvellous, and yet a disappointment. I had gone there to assert my independence, to make sure that what I wrote was *my* thesis, not that of Solow or Samuelson. But I had gone there partly in the hopes that I would find insights into the 'ills' of society which somehow seemed beyond the purview of the neo-classical models which were the focus of MIT economics. I did not succeed. Except for the chapters on income distribution and the paper that was eventually co-authored with Bob Solow, what I wrote had MIT stamped all over it. I had not yet made the break.

2. My early career

Returning to the USA, I spent a year as an assistant Professor at MIT,[10] followed by a regular appointment at Yale's Cowles Foundation as an assistant Professor. Yale was a different place from MIT, and I think it did play an important role in my intellectual development. What made Yale an exciting, if somewhat disconcerting, place was that, unlike MIT, there was not a single view concerning what was good economics. There were those who valued rigour, such as Tjalling Koopmans and Herb Scarf. There were those who valued economic intuition, such as Bill Brainard and Jim Tobin. There were those who tried to blend the two (I suppose all would say that that was what they were doing), including David Cass and Maani Yaari. Interactions were intense, and many joint papers were written.

The intellectual tensions which I had noted at MIT were all the more evident (or should have been) at Yale. There were some who openly professed disdain for macro-economics, because of its lack of rigorous foundations, holding faith to the perfectly competitive model. There were others, such as Tobin, who expressed far more scepticism of the assumptions we employed in our MIT models than I had previously experienced. Giving a seminar was a true experience: while Koopmans might ask you to talk slowly through what seemed to you to be an obvious argument in the beginning of the paper, when you finally came, after much sweat, to the final punch line, Bill Brainard was likely to yell out 'It was obvious'. On one occasion, I set up a friend (Nick Stern) to ask, shortly before he came to the punch line, what the result was. The blank faces made it clear that it was not obvious. Yet, a few seconds later, when he announced the answer, Brainard could not help but blurt out: 'It's obvious'.

I suppose I learned the most during my sojourn at Yale from Jim Tobin, David Cass and Peter Mieskowski. Jim brought a more impressive blend of real-world economics to theory than I had seen before, and he brought deep passion for the problems that economics was supposed to solve. I remember giving a talk on the 'optimal rate of unemployment'. Jim's ears turned red: how could one talk about there being an optimum level of unemployment other than zero!

From Dave, I learned about rigorous mathematical analysis, and under Peter Mieskowski, I became a student of public finance, a 'grand-student' of Richard Musgrave, under whom he had previously studied. As I began my theoretical work in public finance, I approached it with an understanding of the classical questions and modes of analysis, in a way which I otherwise would not have been able to do.

Uncertainty

Though I continued working on the economics of dynamics (Dave Cass and I wrote a paper on ex-post fixed coefficients models, Cass and Stiglitz, 1969, I wrote a paper with Tony Atkinson on localized technical change, Atkinson and Stiglitz, 1969, and one on growth and international trade, Stiglitz, 1970), my interests shifted towards understanding how markets handle uncertainty. My intuition told me that if I was to understand the basic problems of why markets do not behave in the way envisaged by the simple competitive market, it had something to do with problems of uncertainty and information.

In my early explorations, I substantially generalized the Modigliani–Miller theorem, showing that one assumption to which they did not pay attention (the probability of bankruptcy) was crucial, but that many of the other assumptions (such as that there were many firms in the same risk class) were unnecessary (Stiglitz, 1969c). Later (Stiglitz, 1974c), I showed that the mode of analysis could be extended to virtually all of the financial decisions facing a firm. While I was pleased to have established this result with such generality, I was disturbed: I simply did not believe the conclusion. This is one of those cases where the impetus of the theorem was to make one rethink the underlying assumptions, and much of my work over the next few years can be understood in that light.

My collaboration with Michael Rothschild during those years produced some of my most cited papers (Rothschild and Stiglitz, 1970, 1971, 1972). I had been working on a number of problems describing firm and consumer behaviour in the presence of uncertainty. In that work, I wanted to ask a simple comparative statics question: 'How did individuals respond to an increase in risk?' But what did an increase in risk mean? Mike and I sat down to think of all the different meanings that it might have, and then were able to show that, when properly formulated, all of these were equivalent: the concept that came to be called mean preserving spreads.

The issue of comparing probability distributions is closely related to that of ranking income distributions. Both we, and Tony Atkinson, saw this. Using our framework, he provided a new interpretation to our results, that the Lorenz curve for one distribution lies everywhere inside that of the other.[11]

Nairobi

It was at Yale – or perhaps I should say, while I was on leave from Yale – that I began my work on the economics of information. I was invited by the

Rockefeller Foundation to spend some time at the Institute for Development Studies in Nairobi, which I did in the period 1969–71. It was a lucky time to be there, in two respects. It was shortly after independence, and there was an unmistakable exuberance in the country. At the same time, there was a large number of intelligent and devoted expatriates. While they may have coloured my visit, they opened my eyes, and I was able to absorb and understand far more about this country in a short time than I otherwise would have done.

At the time, Nairobi seemed the crossroads of the economics profession. Harris and Todaro (1970) were doing their famous work there. Diamond and Tobin were later to make similar visits. There was a host of expatriates from Britain, many of whom became good friends, including David and Pip Bevan, now at Oxford, Nick Stern, now at the LSE, and Peter Hopcraft, now at the World Bank. I walked the hills around Nairobi with these, as well as with short-term visitors such as Maurice Scott from Oxford.

I focused my attention on the labour market, partly in response to questions being posed to us by the government, but partly from the obvious problems presented by high urban unemployment. It seemed to me that no one in their right mind could hold to a model that assumed that labour markets always cleared (though I seemed, in the end, wrong in that judgement: people who otherwise seemed in their right mind have held that position!). The first question I asked was, how could this be: and out of that grew the efficiency wage theories – the nutritional, the adverse selection, moral hazard and labour turnover versions that I published over the following decade (see, for instance, Stiglitz, 1974d, 1976a, 1982a, 1987a, 1992a; Shapiro and Stiglitz, 1984; Nalebuff, Rodriguez and Stiglitz, forthcoming; Rodriguez and Stiglitz, forthcoming). These theories explained why wages would not fall, even in the presence of an excess supply of labour.

They also led me to think more generally about the nature of the 'market phenomenon' under study, the principles of adverse selection and moral hazard, and more particularly, the problems of imperfect information which underlie them.

Screening and adverse selection

That first summer in Nairobi in 1969, I began three other related projects. The first had to do with hiring skilled workers and the role of the education system. The question had been posed (both by those inside and outside the government): was higher education actually serving to increase the productivity of the economy? Did the higher wages that the more educated received actually reflect a commensurately higher productivity? Gary Fields (1972) had put forward the hypothesis that education was used as a credential; that with rigid ways and an excess supply of workers, firms simply had to have some criterion for choosing who would get hired, and education was as good as any.[12] I re-posed the question: it seemed to me that the problem of an employer was lack of information concerning his employees. Could we say anything about the equilibrium credential and equilibrium wage? In other words, rather than taking the wage as exogenous, I wanted to make it endogenous, and study in that context the role of education as a screening device (rather than just as a credential). That work led eventually to my *American Economic Review* (*AER*) paper (1975b) on *Education as a Screening Device* and, eventually, to my joint paper with Mike Rothschild (1976), where we attempted to understand at a more fundamental level how markets with adverse selection/signalling behave.

In the formulation that Rothschild and I later arrived at (Rothschild and Stiglitz, 1976), we made use of what we called 'self-selection constraints' that the markets provide choices to individuals, such that by their choices, individuals reveal who they are. The high-risk individuals do not buy the insurance deductible; the low-ability individuals drop out of school earlier; the high-ability individuals work longer hours; the owner of the good firm retains more of the shares, the worker planning not to quit accepts a wage pattern with higher wages in the future.[13]

Incentives

The second project involved the analysis of farmer behaviour. Most farmers in Kenya were small landholders; this contrasted with India, and many other parts of the world, where sharecropping was more prevalent. Could Kenya's relative success be partially attributed to the alternative form of land tenure? How could a seemingly inefficient system of land tenure, the sharecropping system, have survived for so long, and over so much of the world? In my analysis (Stiglitz, 1974e), I reversed the standard presumption, which had held that sharecropping attenuated incentives, by showing that in the absence of the ability to monitor workers, they provided the only effective system of incentives going. In so doing, I provided a linear formulation of what is now called the principal–agent problem: the principal (here the landlord) wants to elicit from his agent (the tenant) as much work as he can, and at the same time pay him as little as possible.[14]

Equilibrium wage distributions

The third project had to do with equilibrium wage distributions, a study that I did not really complete until many years later (Stiglitz, 1985c). At the time, I was not exactly sure why price distributions were interesting – other than that they refuted one of the basic laws of economics, the law of the single price. But as our work proceeded, equilibrium price distributions took on added significance; in some cases, unless there was a price distribution, no equilibrium to the market could exist (Salop and Stiglitz, 1982). Moreover, it became clear that 'noise' – here, wage differentials – were not just the result of the fact that the process of arbitrage – of eliminating wage differentials – was costly. Rather, there were inherent processes in market economies with costly search which gave rise to 'noise'. That is, the market created the noise, as firms tried to take advantage of the seemingly little bit of market power which costly search gave them.

This work illustrated a basic theme to which I returned in later work: costly information had a profound effect on the nature of the market equilibrium. This was true even if the costs entailed were very small. And it illustrated one aspect of that theme in particular: that with costly information, there was an 'equilibrium' level of 'disequilibrium' (Grossman and Stiglitz, 1976, 1980a).

(I should relate one incident which shows how similar ideas can arise independently. After I returned to the USA from my first trip to Nairobi in 1969, I had a long conversation with George Akerlof. We had kept in close touch since our graduate student days. After leaving MIT, he had gone off to Berkeley. He had spent a year in India, and we exchanged views both about our experiences, and our ongoing research. He described what was to become his famous Lemons paper (Akerlof, 1970), and I described my sharecropping paper and my paper on education as a screening device. We had both begun formulating some of the basic models underlying information economics.)

Cambridge: second incarnation

In between my two visits to Nairobi, I spent a year at Cambridge, continuing my Fellowship at Caius. The year was a particularly exciting one. Cambridge's established luminaries were still active; they were complemented by a distinguished group of visitors, including Ken Arrow, Roy Radner and Herb Scarf. My young friends from three years earlier – Mirrlees, Bliss, Dasgupta, Heale, Atkinson, Newbery – were proving that they were every bit as good as their initial promise indicated. I began lifelong collaborations with several of them. For instance, while my work with Dasgupta began by re-examining issues of optimal public production and taxation in the presence of restricted taxation, we moved on over the years to discuss issues of development, trade policy, technical change, natural resources and imperfect competition.

World travels

David Lodge, in his book *Small World*, depicts a globe-trotting academia. His academic is an English professor, but soon after I entered the economics profession, I realized the economists made David Lodge's English professor appear sedentary.

My globe-trotting began the summer before accepting my first 'real' job at Yale. I was invited to give a set of lectures at the University of Canterbury in New Zealand (my students, though small in number, were high quality; perhaps the most well-known today is John Riley). On the way back I gave lectures in Australia, Bangkok and Delhi, and visited Cambodia and Nepal. I loved every aspect of these trips: the views of economies so different from the one I had grown up with, the opportunities for intellectual exchanges, the friendships which were made, and have been maintained over the ensuing years.

Long papers

Some of these trips provided me with the occasion to write what I call my 'long papers'. Having decided to give lectures on a particular topic, I sat down and thought about it from varying perspectives. The result was often a long paper 100 pages or more – a genre not favoured by the standard journals. To get the ideas published required breaking up the paper into little parts, a task which I did not find as exciting as going on to think about a new problem.

One example of this was the lecture I gave at the meeting at Hakone organized by Uzawa after the Far Eastern meetings of the Econometric Society, in 1969. I gave an eight-hour lecture entitled 'Some aspects of the pure theory of corporate finance'. (I was, perhaps, not so well attuned as I am today to how much more difficult it is to listen to a lecture than it is to give one.) Over the subsequent years, I published three papers (Stiglitz, 1972b, 1973a, 1974c), and I suspect that there were other good ideas that got lost in the process of cannibalization.

Another example, from the same trip, where it was more than a decade before all the paper was published, was the lecture I gave in Tokyo to the Econometric Society, 'Stockmarket allocation of investment'. In that case, I sent off the whole paper to the *Quarterly Journal of Economics* (*QJE*), but in the usual manner, they insisted on only publishing a paper of the conventional length. In the second half, I had discussed the problem of unanimity of preferences among shareholders, how preferences were affected by whether shareholders were planning to buy or sell shares, and related issues. These issues had not been raised in the earlier literature; they were not among the currently fashionable topics, and naturally, the editors and referees suggested that they be cut. In the subsequent years, they did get raised, and in 1980 the *QJE* did publish a joint paper with Sandy Grossman which revisited these issues. It is my suspicion that the journals in aggregate, and the *QJE* in particular, could have saved considerable space in publishing 'unnecessary' papers if they only let me publish the second half of my Tokyo lecture in 1972.

A third example of a 'long paper' was my paper, 'Alternative theories of wage determination and unemployment in less developed countries'. I wrote this in Nairobi in 1969;[15] it contained many of the various versions of efficiency wage theories that we know today (including the nutritional, the effort and the adverse selection-quality models), though it took more than 15 years for all of them to be published.

Of course, sometimes the delay in getting the various pieces published resulted from the fact that the first version was not as well formulated as it might be. An example of this was provided by my 1971 lecture to the Econometric Society, 'Perfect and imperfect capital markets'. There, I discussed several paradoxes: if capital markets were efficient, what incentive would individuals have to gather information? And how can there be rational speculation? On average, those buying stock on the stock market must make an average return. Thus, if someone makes an above-average return, someone else must make a below-average return. Since the average return has to be average, speculation can only increase risk. The first paradox led to my 1976 and 1980a papers with Sandy Grossman. We showed that there had to be an equilibrium amount of disequilibrium: prices could not fully reflect all the information available to the participants in the market. This provided another example of where the fact that information is costly fundamentally affects how markets behave.

I did not publish my discussion of the second paradox until 1982 (Stiglitz, 1982a), and by then, Aumann's 'common knowledge' ideas had provided a more formal framework for thinking about these issues (see Milgrom and Stokey, 1982).

Oxford: first incarnation

Though my work on the economics of information had begun in Nairobi and Yale, it made enormous progress during a year's sabbatical I took at St Catherine's College, Oxford, in 1973–4, arranged by Nick Stern. Indeed, by the spring of that year, when I was asked to give the annual lecture (Stiglitz, 1975d) to the Association of University Teachers of Economics, work had progressed to the point that I could give a survey (of admittedly largely unpublished results). It was after that talk that Andrew Schulyer of Oxford University Press asked me to convert that lecture, entitled 'Information and economic analysis', into a book, an opus on which I have worked sporadically now for almost two decades. I have often been asked, why the delay in publication? There is no clear answer, but two suggest themselves: one is that as I wrote each chapter, new ideas would occur, which I would pursue. I found it difficult, then, to come back and rewrite the chapter, taking into account the new results. In a sense, the subject grew outside any easily defined bounds. Besides, as time went on, my interests shifted, from the broad principles of the economics of information with which this book was concerned, to more 'applied' problems, such as understanding the macroeconomic phenomena of unemployment and economic fluctuations.

It was during this visit to England that Tony Atkinson and I began in earnest our book *Lectures in Public Economics*, though it was not until many years later that we were able to finish it. It was also during this visit that Dixit (who was then at Warwick, a short distance to the north) and I collaborated on our work on monopolistic competition (Dixit and Stiglitz, 1977).

3. Information economics

From my current perspective, I view my work on information economics as consisting of three phases.

Critique of the prevailing paradigm

In the first, I showed that the standard theory of competitive equilibrium was not robust. Slight changes in the assumptions lead to marked changes in all of its major conclusions: equilibria might not exist (e.g. in the case of adverse selection, Rothschild and Stiglitz, 1976; in the case of moral hazard, Arnott and Stiglitz, 1990; in the case of costly search, Salop and Stiglitz, 1982). In general, competitive equilibrium was not *constrained* Pareto-optimal (i.e. even taking into account the costs of information; see Arnott and Stiglitz, 1989, and Greenwald and Stiglitz, 1986). And when it did exist, many of the standard properties did not hold:

1. The law of single price was repealed.[16]
2. The law of supply and demand was repealed – equilibrium might be characterized by demand not equalling supply (Stiglitz, 1987a). In the labour market, I referred to these theories (growing out of the work that I had begun in Nairobi) as efficiency wage theories, because of their close resemblance to earlier theories in development economics, which argued that wages affected productivity for nutritional reasons[17] – but now, the dependence of productivity on wages arose out of adverse selection or incentive reasons.[18] But the same principles, I realized, applied to credit markets (Stiglitz and Weiss, 1981) and product markets (surveyed in Stiglitz, 1989c).
3. Equilibrium might be characterized by positive profits.[19]
4. Equilibrium might be characterized by non-linear relationships between price and quantity.[20]
5. Equilibrium may be characterized by random contracts.[21]
6. Equilibrium, even with 'free entry', might be characterized by imperfect competition, with every firm having some 'monopoly power'.

The first stage of my work was as much a comment on economics as it was on economies. That is, it provided a critique of the perfect competition, perfect information paradigm that had informed so much of economists' research. It showed that the paradigm was not robust, that one could not have confidence in its conclusions; slight perturbations – small imperfections in information – could have large effects.

The information-theoretic approach to market analysis

The second stage was more constructive. It consisted of applying the basic principles to the study of labour, capital and product markets, to see to what extent the observed nature and form of those markets could be explained by information theory. Of course, understanding how particular markets worked – for instance sharecropping in less developed countries – had provided the original motivation for much of my work in this area. But for several years, I focused my attention on 'insurance' markets, because it seemed to me that they provided the cleanest models, the paradigm cases, for studying both adverse selection and moral hazard.

Having understood how these 'ideal' models worked, I wanted to return to see how the effects I had identified manifested themselves in various contexts. I believed that information considerations could go a long way in explaining the institutional arrangements we observed. While neo-classical theory ignored these arrangements - while it could not explain them, it said that they were essentially irrelevant – I argued that they were both important and explicable, for instance, that output under sharecropping might indeed be lower than if workers owned their own farms, but that sharecropping arose out of a trade-off between risk and incentive considerations.

Labour and financial markets

This research agenda has, I believe, met with considerable success. We now understand why corporate finance matters (contrary to the assertion of Modigliani and Miller); we understand not only the differences between debt and equity, but why firms may be credit rationed and even effectively equity rationed. We understand why firms often base pay on relative performance (see Nalebuff and Stiglitz, 1983a,b). We understand more about the design of contractual arrangements, between firms and their workers (Stiglitz, 1975c), and among firms. Indeed, in classical competitive theory, contracts played no role. Modern information theory reintroduced the role of contracts into economic analysis.[22]

The new theory of the firm

These and related results have led to a new theory of the firm. 'Problems' in the equity market mean that shareholders are imperfectly diversified; these imply that firms act in a risk-averse manner. (See, for example, Stiglitz, 1982a, 1989b; Greenwald and Stiglitz, 1990.) Later, we shall discuss how the theory of the risk-averse firm was an essential building block in constructing a new macro-economic theory.

For decades, economists had spoken of the managerial theory of the firm. The notion that managers acted in the interests of shareholders seemed inconsistent with observations. Yet advocates of the neo-classical model were accused of committing the sin of *ad hoc*ery against those who employed managerial theories: the take-over mechanism would ensure that any firm that was not maximizing market value would be taken over. In work that I began in the late 1960s, I questioned both whether the take-over mechanism would work (Stiglitz, 1972b, 1975d), and whether, if firms did maximize their market value, but there was not a complete set of risk markets, the result would be Pareto-efficient (Stiglitz, 1972a, 1982b). Indeed (Stiglitz, 1970; Grossman and

Stiglitz, 1980b), we went further and showed that not all shareholders would want firms to maximize shareholder values; there would not, in general, be unanimity about shareholders concerning the objective the firm should pursue. All of these strands were developed much more fully in the subsequent literature. There was a spate of work showing the following:

1. Even if there was not a complete set of markets, markets would be efficient (Diamond, 1967) so long as firms maximized stock market value.
2. Firms would maximize stock market value even if there was not a complete set of markets (Leland, 1974; Radner, 1974).
3. Even if firms did not maximize stock market value, so long as they maximized shareholder utility, the market was constrained Pareto-efficient.
4. There was some alternative notion of constrained Pareto-efficiency (social Nash optimality (SNOP), Grossman, 1981, and Grossman and Hart, 1979).

These results were all shown to be very special indeed; they appear now as last-ditch efforts to preserve an existing paradigm. For instance, Diamond's results are that the constrained Pareto-efficiency of stock market economies required no bankruptcy and a single commodity; with two commodities, the results were no longer valid (Stiglitz, 1982b; Greenwald and Stiglitz, 1986); Grossman and I (1977, 1980b) showed how restrictive were the conditions required for all shareholders who wished to maximize market value; my 1970 results, showing that maximizing shareholder utility did not really resolve the problem, so long as some shareholders were either buying or selling shares, had never been directly dealt with by the subsequent literature, which had simply imposed as an equilibrium condition that shareholders were neither buying nor selling shares, a condition which greatly reduced interest in the stock market and its role in the economy; and the restrictions imposed by an alternative criterion, such as SNOP, simply had no relevance to market economies. It was easy to show that even with rational expectations, there existed simple interventions in markets which could improve everyone's *ex ante* expected utility (see also Newbery and Stiglitz, 1982, 1984).

Meanwhile, the take-over literature took off. Among the questions concerning the take-over process that I had raised in my 1974 Association of University Teachers of Economics (AUTE) lecture was how appropriate are the returns of expenditures on information on undervalued firms. The point was simple, and is made most forcefully by way of analogy with the labour market upon which I focused in that lecture. Assume that two workers differ in their ability. In the absence of information, the market will pay them the same wage (representing the expected value of their productivity). I argued that while it might pay the undervalued individual to obtain a credential to persuade potential employers to pay a higher salary – there were fundamental problems with firms undertaking the screening process. If two firms were both to discover that a particular worker was undervalued, they would compete against each other, driving the wage up to the marginal product; all the return to the information would be acquired by the undervalued worker; the firms that had spent their resources to screen the individuals would obtain no return. Only if they had a monopoly on the information could they obtain a return on it. The application of this principle to the stock market is straightforward. Any firm or investor that recognized that some other firm was undervalued could make a killing by buying it. But if two firms obtained that information, all the gains would accrue to the acquired firm. And disclosure requirements make it likely that such information could not be kept secret. The only gains that an acquiring firm could obtain are those from the shares it purchases before it reaches the disclosure threshold.

Grossman and Hart (1980) went beyond this, to look at the incentives of small shareholders who own the shares of a firm to sell their shares, even in cases where the taking-over firm would improve the management of the firm. As had long been recognized, public management is a public good, and the public management of a publicly held corporation is a public good from which all shareholders benefit. (The point is actually more general than this, applying to any class of stakeholders in the firm; see Stiglitz, 1985b.) Small shareholders have no incentive to sell their shares; they can get the full benefits of the improved management, and each believes the decision not to tender shares will have no effect on the outcome.

While the first strand of literature discussed above called attention to the conflicts of interests among shareholders and the lack of a well-defined objective function for the firm, and the second strand of literature emphasized the inability of the take-over mechanism to ensure that firms will maximize sharemarket value, a third strand of literature emphasized that even if the board of directors of a firm could agree on what objectives the firm should pursue, there was no way that they could necessarily ensure that those objectives would be pursued. Managerial discretion meant not only that firms that did not maximize shareholder value would not have their management thrown out, it also meant that managers had the discretion to pursue private interests that were in conflict with corporate objectives. The board of directors had imperfect information concerning what their managers were doing (and managers had only imperfect information concerning what their employees were doing); and, equally importantly, at each level those in charge had only imperfect information concerning what those below them *should* be doing. Indeed, the fact that information is costly is one of the principal reasons that firms must delegate responsibility; and with delegation necessarily comes some loss of control. It was, thus, quite natural that my interests in information economics led me to turn to both the issues of how firms can provide incentives, both to their managers and their employees, and how they can organize the structure of decision-making.

Theory of incentives

The first line of work I initiated with my study of sharecropping (to which I referred earlier), where the problem was how landlords, in their contractual relations with tenants, could balance issues of incentives and risk bearing. As I wrote the paper, I realized not only that it provided a new perspective on this important institution which had been so prevalent in so much of the world for so long, but also that it provided a paradigm for a whole class of such economic relationships. I ended my 1974e paper with a discussion of the applicability of the principles to the organization of modern corporations, as well as with a broad discussion of other control mechanisms - monitoring effort (supervision) as an alternative to monitoring output. As I noted earlier, this paper, along with the contemporaneously written paper by Steve Ross (1973), gave rise to the vast literature now known under the rubric of the principal–agent literature. What is remarkable, to me, is that while economists have long argued that incentives are *the* central economic issue, it was not until this literature, beginning in the early 1970s, that a formal literature addressing the design of incentive structures actually developed.

This issue, of the design of incentive contracts, has blossomed into a rich and vast literature,[23] and I do not want to take this occasion, either to review that literature, or the part that my work may have played in it. For the most part, I saw my goals as:

1. To understand the kinds of incentive structures actually observed.
2. To see what insights this kind of analysis might have for the kinds of institutional arrangements which are observed.
3. To identify some general principles, applicable to a wide variety of situations, rather than to analyse particular parametric forms.
4. To explore what implications the underlying problems posed by incentives had for generally accepted principles of economics.

Let me briefly illustrate each of these aspects.

1. *The design of incentive structures.* Thus, in Stiglitz (1975c), I analysed the design of piece-rate systems as incentive devices, identifying the circumstances under which greater reliance might be placed on piece-rates.
2. *Implications for observed institutional arrangements.* In Braverman and Stiglitz (1982, 1986), we extended my earlier analysis of sharecropping to explain the widely observed practices of interlinking of credit, land and other contracts in less developed countries (LDCs), and to explain observed patterns of cost sharing.
3. *General principles and implications.* In Nalebuff and Stiglitz (1983a,b), we explored the use of contests as incentive devices; and in Arnott and Stiglitz (1986, 1988a), we explored the use of constraints on the availability of insurance, and the use of commodity taxes and other indirect control devices for ameliorating the effects of moral hazard.

4. *Implications for standard theory.* My long-standing collaboration with Richard Arnott has focused on the profound effects that moral hazard (incentive) considerations have for competitive equilibrium analysis. Indifference curves were not, in general, quasi-concave, zero profit loci (between benefits and premium) were not concave, price and income consumption loci were not, in general, continuous – even beginning with the most simple and well-behaved underlying preferences and technologies. A major task over the past decade has been to reconstruct a competitive equilibrium theory, to re-examine issues of equilibrium and existence, as well as all the other standard characterization results.

The theory of the organization of firms

Once one recognizes that the firm might do something other than maximize market value, then it is apparent that what goes on inside the firm could have important economic implications. We had to look inside the 'black box' of the firm. How firms were organized could have important bearings both for economic incentives and for how and what decisions were made. For instance, by organizing firms into competing units, one could use competition to provide incentives, as suggested by my work with Nalebuff.

Another major research project focused not on incentives, but on decision-making. In fact, in this work, undertaken jointly with Raaj Sah, now of the University of Chicago, we ignored incentives completely (see Sah and Stiglitz, 1985a,b, 1986, 1988a,b, 1991). We began with two aphorisms: 'To err is human' (translated: all individuals have limited information, and so have a probability of, for instance, recommending that a lousy project be accepted or a good project be rejected) and 'Two heads are better than one'. We showed how those two heads are organized made a difference: our original objective was to compare the consequences of hierarchically and polyarchically organized decision-making, though in later work we went beyond this to consider the consequences of committees and mixed (e.g. polyarchies of hierarchies) systems. We were able to show that such mixed systems (as found within most capitalist economies) dominate pure systems. We explored not only the implications of the structure of decision-making for the choice of projects, but for the choice of managers/decision-makers: as each generation of managers chooses its successors, we could view organizations as self-perpetuating organizations (Sah and Stiglitz, 1991).[24]

There are many other aspects and implications of organizational design – some more narrowly economic in nature, such as the implications for speed and cost of decision-making (Stiglitz, 1987b, 1989e), some touching on sociology and social psychology, explored more broadly in the vast literature of organization theory – which seem like fruitful areas of research, and to which Sah and I hope to turn in the coming years.

Some general observations about information-theoretic foundations of micro-economics

Two aspects of this research require comment. First, while the theory has drawn attention to institutional details, there are often competing explanations. Thus, Lazear and Rosen (1981) provide an explanation of why pay should depend on relative performance; at its heart, it (like the Nalebuff Stiglitz analysis) is based on an information problem the inability of employers to monitor effort. But as they model the information problem, better solutions could be obtained if the employer were allowed to use non-linear non-relative performance schemes. Simple relative performance schemes are better than *linear* non-relative performance schemes. By contrast, in the model of Nalebuff and Stiglitz, not only is the use of relative performance schemes *not* a consequence of such an artificial restriction, but relative performance schemes have the further advantage of providing what the authors call incentive flexibility, effectively allowing the implied incentives associated with working harder to adapt to changes in economic circumstances, in a way which the employer, because of his limited information, could not do.[25]

This brings me to the second aspect: a strength and weakness of information economics is that the results do seem to depend on the particular assumptions made. It is a strength, because we do see differences in labour, capital and product markets in different situations - in different contexts, at different times. We want to be able to account for these differences. A theory that predicted that agricultural land labour markets would always be characterized by sharecropping would, of necessity, be proven wrong, as economies evolved out of sharecropping. It is a weakness because, to those not in the field, it seems that 'anything can happen', that at the hands of those who have mastered the intricacies of the theory, almost anything can be explained.

To put it another way, we all know what perfect information meant. Information can be perfect in only one way, but information imperfections can take on a myriad of forms. By opening up the possibility of imperfect information, have we opened a Pandora's box?

This criticism is, I think, fundamentally misguided. To be sure, there are many forms of informational imperfections. It is neither surprising nor disturbing that the form of informational imperfection would have effects on the nature of the market equilibrium. It makes no more sense for an economist to say that we shall assume that information is perfect, because we cannot decide the precise way in which it is imperfect, than it does for an engineer to assume that there is no friction, because the magnitude of the friction cannot be estimated precisely.

The dependence of the consequences on the nature of the informational imperfections assumed does give rise to greater complexity. It imposes a stronger burden on the theorist for thinking deeply about whether he has identified the essential informational imperfection in the market in question, and for conducting thought experiments to see how any asserted properties of the equilibrium depend on these informational assumptions. It provides a greater role for empirical work, both to help us choose relevant assumptions and to select among competing models. If economics has become more difficult, it also has become 'richer' – the old adage about an economist being like a parrot, repeating the words 'supply and demand', no longer applies. Economics can say a great deal more about a great many more phenomena.

Stanford: the first incarnation

In describing my research in the economics of information, I have broken it into three stages. Though there is a certain logic to this, it oversimplifies the research process, which, at least in my case, is anything but linear. Thus, while I describe my first stage as being concerned with an attack on the standard paradigm, understanding the limits of the standard paradigm has remained a concern as I have proceeded into the second and third stages. Among the papers to which I attach the most importance are the papers with Bruce Greenwald (Greenwald and Stiglitz, 1986), developing a general methodology to show that economies with imperfect information and incomplete markets are almost always (constrained) Pareto-efficient, and the one with Richard Arnott (Arnott and Stiglitz, 1990) detailing the various sources of market failure which arise in the presence of moral hazard.

Still, I like to think of various parts of my research programme as associated with various stages of my career; and since I have had a fairly peripatetic existence, the various stages of my career are naturally associated with particular locations. Hence, I associate the second stage of my career particularly with Stanford, where I was a Professor between 1976 and 1978.

The California culture – of which Stanford is a part – is a youth-dominated culture, and there is a certain youthfulness even about the more senior statesmen in the department, such as Ken Arrow: a thirst for new ideas and intellectual challenges. It was the kind of environment in which I thrived. As I arrived in Stanford, information economics seemed to have come into its own. I gave whole courses on the subject. Its practical relevance was apparent: one course was given jointly with the business school (illustrating another of Stanford's great strengths: it facilitated, in a way unparalleled by any other institution, interactions across disciplines and schools). It was a great time for information economics, both from the perspective of Faculty and students. Among the Faculty members that were hired while I was there were Michael Spence and Sandy Grossman. Ken Arrow had, of course, pioneered in the area, and Bob Wilson had been exploring the implications of asymmetric information in auctions. Among the students in my class were David Kreps, Andy Weiss, Avi Braverman and Richard Gilbert – the last three of whom became co-authors. Among papers written for that course was the one Kreps subsequently published in the *Journal of Economic Theory* (*JET*) (1977).

During this period, I continued two other interests. The oil crisis in 1973 had focused attention on natural resources. Gerry Brannon, of Georgetown, was in charge of a large Ford Foundation study of energy policy, and he had asked me to do a study of tax policy. This led to another one of my 'long papers', parts of which were subsequently published (Stiglitz, 1974a,f, 1975a, 1982b). Shortly thereafter, an extremely fruitful collaboration began on these issues with David Newbery, Partha Dasgupta and Geoff Heal, all of whom visited Stanford during this period, and with Richard Gilbert, who was then completing his thesis under me. This collaboration entailed two major consulting projects, one for the Federal Energy Administration, examining biases in the markets' intertemporal allocation of exhaustible natural resources, and the other for the Electric Power Research Institute, examining, among other issues, the consequences of imperfect risk markets for the allocation of resources to energy. These consulting tasks were ideal: they both made use of recent advances in economic theory, and they forced us to think hard about basic issues, within a concrete setting. Much of our attention was directed to understanding how markets with imperfect competition work, and this work influenced much of my subsequent work in this area.

Over the years, I have been lucky in getting many consulting projects that have been extremely complementary with my ongoing research programme. Thus, in this period, Darius Gaskins, who had taught at Berkeley and become Assistant Secretary for Policy and Evaluation in the Department of Interior, asked Bob Wilson and me to think about various parts of the government's oil and gas leasing programmes. Bob did work on the design of the auction, and I did work on exploration which could be viewed as a problem in allocating resources to gather information.

The other major research programme which I began in this period involved technical change. My interests in technical change dated back to my MIT days, my summer in Chicago with Uzawa, and my year in Cambridge. While at Yale, I first modelled a patent race (in my 'long paper' on perfect and imperfect capital markets, 1971), and considered its efficiency; I attempted to characterize the biases in the speed of innovation. Work on technical change was quite naturally related to my other interests: expenditures on technical change could be viewed as expenditures to acquire a particular type of knowledge (information), and many of the 'problems' that the economy had in dealing with information arose also in the context of technical change. Both technical change and imperfect information helped explain what was wrong with the basic competitive model, why it did not provide a good description of modern industrial economies. As Schumpeter had emphasized earlier, markets in which technical change was important were necessarily imperfectly competitive. Unfortunately, economists had not developed good models analysing the nature of competition in markets where technical change was important.

It was at Stanford that technical change became a major focus of interest. This was not surprising: Silicon Valley was one of the great centres of technical change in the world, and Stanford had some of the greatest students of technical change in the world, such as Paul David and Nate Rosenberg. To understand technical change, one had to look at what was going on at the micro-economic level, or even within the firm. Spence, David and I got together to get a research grant from a division of the National Science Foundation which was supposed to focus on issues of technical change. They put us through many hoops to get the grant: the long proposal we eventually wrote would, I think, have been publishable as an original monograph on the subject. Partha Dasgupta, Paul David and I also began our collaboration on a project that we eventually hoped to turn into a book on the subject, though all of us have been sidetracked, over the years, on to other projects. In 1977 Partha and I presented a lecture to the International Economic Association meeting in Tokyo on the relationship between market structure and technical progress (eventually published as Dasgupta and Stiglitz, 1980a,b). Though when we began our work, there was only limited interest in technical change, we were pleased to see that during the 1980s, this area finally began to get the attention it deserved.

The third stage: constructing a macro-economic model

The third stage, upon which much of my current work focuses, involves using the results of the second stage – the understanding of labour, capital and product markets – to develop a model of macro-economic equilibrium.[26]

The disparity between macro-economics and micro-economics has long been recognized. While micro-economists accused macro-economists of *ad hoc*ery (in the lexicon of theorists, one of the cruellest charges that can be hurled against anyone), macro-economists accused micro-economists of irrelevance. While the central theorem of micro-economics, the fundamental theorem of welfare economics, lauded the virtues of the capitalist system, macro-economists were concerned with its problems, including the marked fluctuations of economic activity and employment which had characterized capitalism throughout its history.

In recent years, two strands of literature – the new classicals and real business cycle theories – have tried to reconcile the two by making macro-economics like micro. But, to a large extent, the micro-economics which they adopted was the micro-economics of the pre-1970s. It was the perfect information–perfect markets micro-economics, which the work of the 1970s and 1980s had shown to be seriously flawed. Thus, while the fundamental theorem of welfare economics had shown that competitive economies were (under certain conditions) Pareto-efficient, Greenwald and Stiglitz (1986) had shown that, with few exceptions, whenever markets were incomplete (as risk markets and future markets obviously were) and whenever information was imperfect (as it obviously was), the economy was not constrained Pareto-efficient.

In a sense, the observations of periodically high levels of unemployment (in spite of the *assumption* of market clearing in the competitive model), high volatility in economic activity (in spite of the buffers and shock absorbers associated with the competitive economy), and that money mattered (contrary to the classical dichotomy) provided empirical refutation of the competitive paradigm. The neo-classical synthesis, which underlay much of the profession's thinking in the decades after Keynes - the contention that once the problems of unemployment were cured, the old classical propositions prevailed - had no scientific basis behind it; it was an assertion, equivalent to the hypothesis that if the patient is not desperately ill, he must be in perfect health.

I, and my co-authors, in particular Bruce Greenwald, now of Columbia, and Andy Weiss, now of Boston University, believed that macro-economics and micro-economic theories could be reconciled, not by trying to adapt macro-economics to the outmoded perfect market models, but rather by using the models of labour, capital and product markets based on imperfect information which we had developed over the preceding decade. Efficiency wage theories could help explain wage rigidities and unemployment; information problems in the capital market could help explain credit rationing, and why firms made limited use of equity markets; the limited use of equity markets could, in turn, explain the risk-averse behaviour of firms; and risk-averse behaviour of firms could, in turn, explain why their behaviour was sensitive to changes in firms' net worth and asset positions, as well as to expectations; this in turn, could help explain economic volatility.

We developed models helping to explain price and wage rigidities, but at the same time, we suggested that the price rigidities might not be at the core of the economy's macro-economic problems. With unindexed loan contracts, greater price flexibility might have made matters worse.

This research programme is far from completed, but results obtained so far make us confident that the factors we have identified are at least an important part of the explanation of the central macro-economic phenomena.

Oxford, Princeton and Stanford: the second incarnation

The third stage of my research began during my second visit to Oxford, where I was appointed the Drummond Professor in 1976. As good luck would have it, this was a high period in the intellectual life in the economics community at Oxford. Jim Mirrlees had just migrated from Cambridge, and his influence was being immediately felt. Mirrlees was not only a great intellect, he was also an institution builder. He attracted others to Oxford, and made it an exciting place. I was also grateful for his steering so many of the best students at Oxford to work with me: in the three short years I was there, these included Franklin Allen, Ravi Kanbur, Margaret Bray, Joe Farrell and several others who have gone on to have distinguished careers in economics.

Since my first teaching at Yale, I had viewed micro-economics and macro-economics as inextricably linked together. I did not want to be pigeon-holed as either a macro-economist or a micro-economist. At Oxford, professors had a great deal of leeway, both in choosing what to lecture on and when to lecture. I had fond memories of my eight-hour Hakone lecture. I saw great advantages in the total immersion in a subject. I was persuaded, however, that this format might have some pedagogic disadvantages, and so I compromised by giving four four-hour lectures, presenting the micro-economic foundations of macro-economics, and the macro-economic consequences of market failures arising out of imperfect information.

It was during this period that I began visiting Bell Labs on a regular basis, which I continued through my years at Princeton. I was involved in a joint research project with Andy Weiss, and soon thereafter began working with Bruce Greenwald, who had completed his thesis on the consequences of imperfect information for the working of the labour market. We had independently arrived at a number of similar ideas (e.g. concerning both the causes and consequences of imperfections in the equity market); thus began a fruitful collaboration that has lasted over a decade. The three of us are currently working on a book trying to set out these ideas in a coherent and complete way.

Here, I should mention my collaboration with Carl Shapiro. Our joint work began as he was finishing his thesis on reputation models at MIT. The similarity between this reputation model and the incentive-based efficiency wage theory that I was trying to formalize became apparent, and we embarked on a joint project that eventually led to our 1984 paper. It also began a collaboration that has continued, including working together editing the *Journal of Economic Perspectives.*

While I was at Oxford, I was asked (with David Newbery) by USAid to do some work on commodity price stabilization schemes. The work I had done on risk had attracted some attention, and price volatility was obviously an important problem in many LDCs. I am sure we gave them more than they expected: we wound up producing several articles and a rather long book that was published by Oxford University Press (Newbery and Stiglitz, 1981).

Of the new projects I began at Princeton, four stand out. The first was on the theory of moral hazard, with Richard Arnott. We had collaborated on a variety of issues ever since his student days at Yale. Back in the early 1970s we had had discussions on (and Richard had written about) randomization with moral hazard and adverse selection. At this point, I cannot recall the immediate impetus for our revival of interest in moral hazard: I suspect it was some work that Richard was doing analysing labour markets. As so often seemed the case, we began with a concrete problem – trying to understand the design of contracts with labour turnover costs (Arnott and Stiglitz, 1985); but this quickly led us to try to formulate a canonical model in which we could isolate the basic issues. These turned out to be far more complex, and interesting, than we had fully anticipated, and gave rise to a long sequence of papers (e.g. Arnott and Stiglitz, 1986, 1988a, 1990; Arnott, Hosios and Stiglitz, 1988).

The second was on organizations, with Raaj Sah, that I described earlier. The third, also with Raaj Sah, was concerned with taxation in developing countries, though the general models also provided insights into, for instance, the 'size of the scissors' (taxation of the rural sector) in the years after the Bolshevik Revolution. (The collaboration gave rise to a number of joint papers, and a book, *Peasants versus City Dwellers*, Sah and Stiglitz, 1992.) From my side, the impetus for this research was a visit by a Chinese delegation to Wingspreads (Wisconsin) in 1980, shortly after the end of the Cultural Revolution, and a return visit I made to China in 1981. The issue of what should be the size of the scissors was high on their agenda, and I realized that we had no analytic framework to give them an answer. Sah was doing work on this issue at the World Bank; Avi Braverman (who was then at the World Bank) put us into contact, and thus began an extremely fruitful collaboration.

The fourth project was on the theory of rural organization (partly with Avi Braverman and Karla Hoff). It was apparent that the imperfections of information on which I had been focusing were perhaps even more important in LDCs, and that studying LDCs through these lenses could help us understand the institutions within the LDCs (Stiglitz, 1985a, 1988). By the late 1980s, this view came to be widely shared, and a number of empirical studies viewing institutions (such as credit and land markets) within LDCs from this perspective were under way. We organized a conference[27] at Annapolis which, we hope, will help define the emerging discipline of the economics of rural organization.

While I was at Princeton, I also became involved in perhaps the largest (in dollar value) litigation that I have participated in, to date. (The out-of-court settlements exceeded $4 billion.) And it illustrates the curious way an idea, written even in an obscure place, can have consequences long after it is written. In my 1973 energy policy piece, I had discussed the issue of information externalities in the context of oil drilling. And information externalities became the focus of the dispute between the states of Texas and Louisiana and the federal government concerning the disposition of revenues from the lease of hydrocarbons on certain offshore tracts. I enjoyed explaining the idea of information externalities to the court, and was pleased when the court ruled in favour of the state of Texas, basing its decisions largely on the kinds of theoretical considerations I had put forward. I have always thought that out of any such consulting experience, one ought to be able to generate some lessons of general interest; I published a paper with Jeff Leitzinger based on this case (Leitzinger and Stiglitz, 1984).

During this period, the Reagan administration, under Secretary Watts, was actively engaged in trying to turn over as much land to private developers as possible, regardless of the cost to the public. We tried (through Governor Mark White of Texas) to get the courts to enjoin the 'fire sales'. Though the attempt was unsuccessful, the process (including the analytic work we did to support our contentions) was exciting: a different way of trying to influence policy.

When I returned to Stanford in 1989 – having been away for 12 years – much had changed, and yet much had remained the same. There were many familiar faces. My student at Yale, John Shoven, was now Department Chairman. Jim Rosse, who had hired me in 1974, was now Provost of the University. Though Ken Arrow was nearing retirement, he had not lost any of his verve, and he turned department meetings into interesting seminars. Bob Hall, a contemporary at MIT, had joined the faculty some years before. John Taylor had been at Princeton with me. But there were many new faces, many of whom shared many interests with me, including Paul Milgrom and Masa Aoki. It has been an exciting place, with many people working in each of the areas in which I have had interests, and with a group of extremely bright graduate students.

4. Other research programmes

Most of my research over the past quarter of a century has been focused around trying to understand the implications of incomplete risk markets and imperfect information for how the economy behaves. But there were several other concerns.[28] Some of these I have already mentioned – such as development and technical change. There are two others which I wish to mention briefly.

Comparative economic systems

In my graduate student days, we had debated the merits of socialism versus capitalism.[29] In later years, ideas of worker co-operatives and other forms of economic organization had attracted my attention. (I participated in a set of seminars, organized by the Acton Society, in Italy and the UK on these issues in the 1970s.)

The collapse of socialism in Eastern Europe raised two sets of issues: first, as the former socialist economies made a transition to capitalism, they had to design a whole new set of economic institutions. This process highlighted a variety of facets of capitalism, upon which traditional theory had placed little emphasis, but which had been the centre of discussion of much of the micro-economics of the 1970s and 1980s: issues of corporate governance, competition policy, bankruptcy law and the design of financial institutions. The issue of privatization – the distinctions between private and public ownership – I had studied with David Sappington in work done at Bell Labs (Sappington and Stiglitz, 1987); I now re-examined these issues, from a much more practical point of view (Stiglitz, 1992d).

Secondly, long ago, there had been a debate between Hayek and Lange about market socialism. Lange took seriously the standard competitive model; Hayek argued that there was far more to market economies than was captured by the Walrasian model – which seemed to suggest that prices could convey all the relevant information between consumers and firms – and that central planning simply would not work. The information economics of the 1970s and 1980s enabled us to see this debate in a new light, to understand more fully the flaws in the Walrasian conception. In my Wicksell lectures, 'W[h]ither socialism?' (1992e), I tried to develop these ideas, to understand more fully how capitalist economies function, the role of competition, property rights and profits.

The economic role of the state

A central issue with which I have been concerned throughout has been the appropriate role of the state. No student of history – particularly the history of the twentieth century – could doubt that the state can do great harm. But whether markets, by themselves, would attain a Pareto-efficient allocation of resources (as many free market economists seem to claim) is certainly more problematical. At one level, much of my research has been addressed to understanding the limits of markets. But to balance this, one needs to understand the limits – and strengths – of the government. Thus, when Arnold Heertje asked me to write an extended essay on the *Economic Role of the State* (Stiglitz, 1990), I willingly agreed. In that essay, I tried to explain what distinguished the state from other economic organizations, and how the strengths and limitations of the government derived from those distinct characteristics.

I have now begun a research project, with the World Bank, which is concerned with addressing a similar question in the context of development. What is, or should be, the role of the state in promoting economic development? How do these questions need to be re-examined in the light of the 'Asia miracle'?

5. Concluding remarks

On methodology

Let me say a few words about methodology and research strategies. It seems to me that for the most part, insights in economic analysis have not come from developing the most general models with which to address a problem. To be sure, in some cases, we may be misled by *oversimplification*: stock market models with a single commodity and multiplicative uncertainty are constrained Pareto-efficient, while with two commodities they are not; so, too, moral hazard models with a single commodity are constrained Pareto-efficient, but not if there are two commodities. One simply cannot make progress in understanding capital markets if one ignores bankruptcy.

But would one learn much more from a model of many commodities, or one in which there is a continuum of commodities? To be sure, often one cannot answer this question until one does the research – though an understanding of economic structure may provide a good hint at an answer.

Did the generalizations of general equilibrium theory, which allowed the proof of existence of competitive equilibrium under weaker conditions, enhance our understanding of how competitive markets function, or of the kinds of conditions under which they did not function? Or were the insights provided by simple models focusing on the problems of imperfect information – informational assumptions which were not even listed among the key assumptions entering into Debreu's theory of value – of more fundamental importance in understanding the limitations of that model?

To be sure, there is virtue in generality: one would like to obtain the most general results under the weakest possible conditions. But economists, of all people, should be aware of trade-offs. To obtain generality in some dimension, we may lose generality in others. The general equilibrium model is not the most general model, for it makes very specific, and very unrealistic, assumptions concerning information.[30] To obtain generality in some directions, e.g. in the number of commodities or the functional forms used, one has to give up generality in other directions, e.g. concerning the realism of the information assumptions.

Different economists may differ in their judgements about the most likely direction of fruitful research, even when they might agree upon the shape of the ultimate model that one would like to construct. Thus, one might agree that eventually one would like to have a dynamic general equilibrium model incorporating all the relevant forms of informational imperfections accurately modelled. But it is unlikely that such a model would emerge in our lifetime. And even if we were successful in constructing and analysing such a model, how useful would it be? We must remember the basic lesson we tell our students: models are meant not to be an exact replication of the economy, but an attempt to distil the essence of the phenomena under study. The issue is, what is the essence?

Here, I think much of the profession has been misled by an overemphasis on generality. (Certainly, we must test the robustness of our conclusions; and I have already provided examples where oversimplifications have been misleading.) By the same token, too much reliance has been placed on accepted conventions – we know what we mean by perfect information, but imperfect information opens a Pandora's box of possibilities! To be sure, when we walk into these new territories, our ground may not be as firm; we may not be as confident that we are making the right assumptions; but surely that must be better than making assumptions that we *know* are wrong.

On teaching and collaboration

I have been lucky during the past 25 years to have had a large number of collaborators. When I was first embarking on an academic career, my teacher Arnold Collery talked about the loneliness of research. Most of our thinking is done alone. I enjoy the parts of each day I spend alone, writing and thinking, but I also enjoy immensely the interactions that collaborative research provides, the give and take of debate, of interchanging ideas, of working and struggling to understand some seemingly intractable puzzle. The friendships of which these intellectual exchanges are a part have been an important part of my life.

I have spent most of this essay describing my research. Yet I have spent much of my time during the past quarter century teaching, and in fact the two are inextricably linked. I have learned much from my students. They have challenged me, and I have tried to bring out the best in each. I have tried to emulate the examples of the great teachers that I had, at Amherst, MIT and Cambridge, emphasizing the posing of questions, the importance of creativity, the struggle for an understanding in the broadest sense possible. Many of these teacher–student relationships developed into lifelong friendships and collaborations.

I look back now with pleasure on the Saturday in April 1963 when I decided to become an economist: the intellectual excitement of new ideas, and the satisfaction of an increased understanding of how economies function. When I entered economics, it was partly because of a concern to be 'socially relevant'. With age comes cynicism. Yet, it is still my hope that some of this increased understanding may have led, or may lead in the future, to a better society, particularly for those living in the Third World. The wonderful part of our profession is that each answer leads to more questions: we create our own demand. I am bullish on economics: we have made enormous progress in our understanding of the economy in the last two decades. There is every reason to believe that the coming years will be equally exciting.

Notes

1. In those days, an oral examination was still part of the comprehensives. Samuelson was on the committee. I recall him asking an obscure question that was discussed in one of his unpublished papers. When I hesitated slightly in answering, he remarked (in a not unkindly way) that he used to think there were two people who knew his work, now he thought there might only be one.
2. There are many anecdotes that could be told about those years at MIT. The summer after my second year, Hirofumi Uzawa invited a group of us out to Chicago (including Sheshinki, Akerlof, Datta Chaudhuri, and an Italian visitor, Georgio Lamalfa and his beautiful wife, Eva). Together with Frank Levy, who had come from Yale, and perhaps one or two others, we rented a couple of apartments near the University of Chicago, and met with day-long seminars for an entire month in Chicago. It was a thrilling experience. Uzawa displayed the same passion for economics that I was later to see in the Cambridge economists. At the end of the month, several of us moved on to George Akerlof's house on the shores of Squam Lake, where we continued our debates and our friendships. And we were joined by some Cambridge friends, Tom Weiskopf and his wife, and Steve Marglin and his wife. Sadly, at the end of the summer, some marital rearrangements seemed to occur – my first exposure to the instability of married life.
3. For several of the essays produced during that summer, see Shell (1967).

4. The paper was published as Stiglitz (1967).
5. This chapter was published (in part) as Stiglitz (1973b). For a more extensive discussion of my views of the Cambridge–Cambridge controversy, written after I had left both Cambridges, see Stiglitz (1974b).
6. I took this theme up once again in a discussion of the desirability of transfer taxes for stocks. See Stiglitz (1989a).
7. I understand in some other branches of science similar phenomena arise. One could not explain the flying of an aeroplane on the basis of 'perfect' models of air flows.
8. I followed this initial work with some subsequent work with David Bevan (Bevan and Stiglitz, 1979), modelling the intergenerational transfer of abilities, and some other work (Stiglitz, 1976b, 1976c, 1978) focusing more on inheritances. Loury (1981) and others have used the structure to explore the special problems posed by human capital.
9. He was a man of enormous determination and courage. He had been struck by polio and, though partially paralysed, led an active life. He passed on his intellectual creativity to his son Joe, whom I had the good fortune to teach during my sojourn in Oxford.
10. MIT had (and still has) a policy against hiring its own graduates. They made a partial exemption, by allowing me to teach for a year, while I looked for a job at an American university. There is an apocryphal tale (actually true) about my 'contract' which stipulated that I had to wear shoes and had to have a place to sleep other than my office. During the last few months I was a graduate student at MIT, it did not seem to make sense to rent an apartment, since I knew I was leaving the country shortly, and since I could minimize travel time by sleeping in my office. I found the rugs quite comfortable; there were showers in the Faculty club on the sixth floor, and the night watchman provided companionship.
11. Our 1973 paper addressed the issue of income distribution, and provided alternative, much simpler proofs for the simpler forms of distributions associated with income (Rothschild and Stiglitz, 1973).
12. I am not really doing full justice to the subtlety of his argument. His paper was original and extremely thorough, and touched on many issues beyond those which I can mention here.
13. I returned to Yale with my basic 'adverse selection model' and had the good fortune to have some brilliant students – particularly Steve Salop – who saw the importance of this issue, and how the kind of model could be applied to a variety of situations. In doing so, he not only extended the model, but introduced some of the basic vocabulary that we use to discuss these issues today. (See e.g. Salop and Salop, 1976 and Salop and Stiglitz, 1979.) The latter paper, for instance, used the model to analyse limit pricing behaviour.) We learned much from each other.
14. It was only later that it became apparent that Mirrlees's (1971) optimal tax problem and Ross's (1973) agency model were all essentially equivalent problems.

    At approximately the same time, in a much more abstract context, Hurwicz began emphasizing the role of incentive compatibility constraints – constraints that we now recognize are formally equivalent to the self-selection constraint of our theory.
15. I left it behind to be typed when I left at the end of the summer in 1969, taking with me only a handwritten earlier draft. I became concerned when I did not receive the typed version after several months. It was not until I returned in the summer of 1971 that I was finally able to recover it.
16. For the labour market, see Stiglitz (1985c); for the product market, Salop and Stiglitz (1977, 1982).
17. The literature in this area has become vast. For a survey, see Katz (1986).

18. Or because of the dependence of labour turnover on wages, as in Stiglitz (1974c).
19. In the context of moral hazard, see e.g. Arnott and Stiglitz (1990).
20. As in Rothschild and Stiglitz (1976) or, in the case of imperfect competition, Stiglitz (1977). Here, non-linear prices enable the firm to price-discriminate effectively.
21. That is, aspects of the contract might be determined only after the contract was signed, though at the time of signing, the individual would know the probability distribution of these terms. See Arnott and Stiglitz (1988b).
22. To my knowledge, the first example of this was in my 1974c paper, though I am sure that others had independently made use of similar constructs. For a more general discussion of contracts see Stiglitz (1992c).
23. For a brief summary of my views on certain aspects of this literature, see Stiglitz (1989d).
24. In one line of research, not yet completed (Sah and Stiglitz, 1985b), we examined the implications of evolutionary selection processes for the survival of organizations designed in different ways.
25. Another example of competing explanations of institutional arrangements is the contrast between Cheung's (1969) explanation of sharecropping, based on transaction costs theory, and my explanation (1974e) based on incentives and risk sharing. For a discussion, see Stiglitz (1987c).
26. For an early survey of some of this work, see Greenwald and Stiglitz (1987b); for a more recent survey of some aspects, see Stiglitz (1992b). Other papers include Stiglitz and Weiss (1992), Greenwald and Stiglitz (1987a, 1988a, b, c, 1989, 1992).
27. The proceedings of this conference are published as Hoff, Braverman and Stiglitz (1992).
28. I mentioned my work with Sah on taxation in LDCs, with Newbery on commodity price stabilization, and on various aspects of rural organization, including sharecropping, at various times in collaboration with Braverman, Hoff and Newbery. I should also mention work on cost-benefit analysis (some joint with Dasgupta and Charles Blitzer) and on sovereign debt (joint with two former students, Mark Gersovitz and Jonathan Eaton).
29. I had the good fortune to listen to Oskar Lange lecture in 1963 and to meet him. In 1965, I went to Poland, to talk to Kalecki and other scholars at the Central Institute for Statistics.
30. As we noted earlier, the fact that perfect information can by definition take on only one form, while imperfect information can take on a variety of forms, should not be taken as evidence that perfect information is a better assumption. Would an aeronautical engineer, uncertain about the magnitude of air resistance, be better off assuming it were zero?

References

Akerlof, G. (1970) The market for 'lemons': quality, uncertainty and the market mechanism. *Quarterly Journal of Economics*, **84**(3), 488–500.

Arnott, R., Hosios, A. and Stiglitz, J. E. (1988) Implicit contracts, labor mobility and unemployment. *American Economic Review*, **78**(5), 1046–66.

Arnott, R. and Stiglitz, J. E. (1985) Labor turnover, wage structure and moral hazard: the inefficiency of competitive markets. *Journal of Labor Economics*, **3**(4), 434–62.

Arnott, R. and Stiglitz, J. E. (1986) Moral hazard and optimal commodity taxation. *Journal of Public Economics*, **29**, 1–24.

Arnott, R. and Stiglitz, J. E. (1988a) The basic analytics of moral hazard. *Scandinavian Journal of Economics*, **90**(3), 383–413.

Arnott, R. and Stiglitz, J. E. (1988b) Randomization with asymmetric information. *Rand Journal of Economics*, **19**(3), 344–62.

Arnott, R. and Stiglitz, J. E. (1989) The welfare economics of moral hazard. In H. Louberge (ed), *Risk, Information and Insurance: Essays in the Memory of Karl H. Borch*. Norwell: Kluwer, 91–122.

Arnott, R. and Stiglitz, J. E. (1990) Equilibrium in competitive insurance markets with moral hazard. Working Paper.

Atkinson, A. B. and Stiglitz, J. E. (1969) A new view of technological change. *Economic Journal*, **LXXIX**, 573–8. (Papers and Proceedings of the AEA Meetings, Chicago, 1968.)

Barro, R. J. and Grossman, H. I. (1971) A general disequilibrium model of income and employment. *American Economic Review*, **61**, 82–93.

Bevan, D. and Stiglitz, J. E. (1979) Intergenerational transfers and inequality. *The Greek Economic Review*, **1**(1), 8–26.

Braverman, A. and Stiglitz, J. E. (1982) Sharecropping and the interlinking of agrarian markets. *American Economic Review*, **72**(4), 695–715.

Braverman, A. and Stiglitz, J. E. (1986) Cost sharing arrangement under sharecropping: moral hazard, incentive flexibility and risk. *Journal of Agricultural Economics*, **68**(3), 642–52.

Cass, D. and Stiglitz, J. E. (1969) The implications of alternative saving and expectations hypotheses for choices of technique and patterns of growth. *Journal of Political Economy*, **77**, 586–627.

Cheung, S. (1969) *The Theory of Share Tenancy*. Chicago: University of Chicago Press.

Dasgupta, P. and Stiglitz, J. E. (1980a) Industrial structure and the nature of innovative activity. *Economic Journal*, **90**(358), 266–93.

Dasgupta, P. and Stiglitz, J. E. (1980b) Uncertainty, industrial structure and the speed of R & D. *Bell Journal of Economics*, **11**(1), 1–28.

Diamond, P. (1967) The role of the stock market in a general equilibrium model with technological uncertainty. *American Economic Review*, **57**, 759–76.

Dixit, A. and Stiglitz, J. E. (1977) Monopolistic competition and optimal product diversity. *American Economic Review*, **67**(3), 297–308.

Fields, G. (1972) Towards a model of education and labor markets in labor surplus economies. Growth Center mimeo, Yale University.

Greenwald, B. and Stiglitz, J. E. (1986) Externalities in economies with imperfect information and incomplete markets. *Quarterly Journal of Economics*, **C1**(2), 229–64.

Greenwald, B. and Stiglitz, J. E. (1987a) Imperfect information, credit markets and unemployment. *European Economic Review*, **31**, 444–56.

Greenwald, B. and Stiglitz, J. E. (1987b) Keynesian, new Keynesian and new classical economics. *Oxford Economic Papers*, **39**, 119–33. (Subsequently reprinted in P. J. N. Sinclair (ed), *Price, Quantities and Expectations*. Oxford: Oxford University Press, 1987, 119–33.)

Greenwald, B. and Stiglitz, J. E. (1988a) Examining alternative macroeconomic theories. *Brookings Papers on Economic Activity*, **1**, 207–70.

Greenwald, B. and Stiglitz, J. E. (1988b) Imperfect information, finance constraints and business fluctuations. In M. Kohn and S. C. Tsiang (eds), *Finance Constraints, Expectations and Macroeconomics*. Oxford: Oxford University Press, 103–40.

Greenwald, B. and Stiglitz, J. E. (1988c) Information, finance constraints and business fluctuations. In (Proceedings of Taipei) *Symposium on Monetary Theory*, Institute of Economics, Academia Sinica, 299–336.

Greenwald, B. and Stiglitz, J. E. (1989) Toward a theory of rigidities. *American Economic Review*, **79**(2), 364–69.

Greenwald, B. and Stiglitz, J. E. (1990) Asymmetric information and the new theory of the firm: financial constraints and risk behavior. *American Economic Review*, **80**(2), 160–5.

Greenwald, B. and Stiglitz, J. E. (1992) Financial market imperfections and business cycles. *Quarterly Journal of Economics*.

Grossman, S. (1981) Nash equilibrium and the industrial organization of markets with large fixed costs. *Econometrica*, **49**(5), 1149–72.

Grossman S. and Hart, O. (1979) A theory of competitive equilibrium in stock market economies. *Econometrica*, **47**, 293–330.

Grossman S. and Hart, O. (1981) The allocational role of takeover bids in situations of asymmetric information. *Journal of Finance*, **36**, 253–70.

Grossman, S. and Stiglitz, J. E. (1976) Information and competitive price systems. *American Economic Review*, **66**(2), 246–53.

Grossman, S. and Stiglitz, J. E. (1977) On value maximization and alternative objectives of the firm. *Journal of Finance*, **XXXII**(2), 389–402.

Grossman, S. and Stiglitz, J. E. (1980a) On the impossibility of informationally efficient markets. *American Economic Review*, **70**(3), 393–408.

Grossman, S. and Stiglitz, J. E. (1980b) Stockholder unanimity in the making of production and financial decisions. *Quarterly Journal of Economics*, **94**(3), 543 66.

Hahn, F. (1966) Equilibrium dynamics with heterogeneous capital goods. *Quarterly Journal of Economics*, **80**, 133–46.

Harris, J. R. and Todaro, M. (1970) Migration, unemployment, and development. *American Economic Review*, **60**, 126–42.

Hoff, K., Braverman, A. and Stiglitz, J. E. (1992) *Economics of Rural Organization*. Oxford: Oxford University Press.

Katz, L. (1986) Efficiency wage theories: a partial evaluation. In S. Fischer (ed), *NBER Macroeconomics Annual 1986*. Cambridge, Mass.: MIT Press.

Kreps, D. M. (1977) A note on fulfilled expectations' equilibria. *Journal of Economic Theory*, **14**, 32–43.

Lazear, E. P. and Rosen, S. (1981) Rank-order tournaments as optimum labor contracts. *Journal of Political Economy*, **89**(5), 841–64.

Leitzinger, J. and Stiglitz, J. E. (1984) Information externalities in oil and gas leasing. *Contemporary Policy Issues*, **5**, 44–57. (Paper presented at the Western Economic Association Meetings, July 1983.)

Leland, H. (1974) Production theory and the stock market. *Bell Journal of Economics and Management Science*, **5**(1), 125–44.

Levhari, D. (1965) A nonsubstitution theorem and switching of techniques. *Quarterly Journal of Economics*, **79**, 98–105.

Loury, G. C. (1981) International transfers and the distribution of earnings. *Econometrica*, **49**(4), 843 67.

Milgrom, P. and Stokey, N. (1982) Information, trade and common knowledge. *Journal of Economic Theory*, **26**, 17–29.

Mirrlees, J. (1971) An exploration in the theory of optimum income taxation. *Review of Economic Studies*, **38**, 175 208.

Nalebuff, B., Rodriguez, A. and Stiglitz, J. E. (forthcoming). Equilibrium unemployment as a worker screening device. Paper presented at NBER CEPR Conference on Unemployment and Wage Determination, Boston, October 1991, forthcoming in the Proceedings.

Nalebuff, B. and Stiglitz, J. E. (1983a) Prizes and incentives: towards a general theory of compensation and competition. *Bell Journal* **14**(1), 21–43.

Nalebuff, B. and Stiglitz, J. E. (1983b) Information, competition and markets. *American Economic Review*, **72**(2), 278–84.

Newbery, D. and Stiglitz, J. E. (1981) *The Theory of Commodity Price Stabilization*. Oxford: Oxford University Press.

Newbery, D. and Stiglitz, J. E. (1982) The choice of techniques and the optimality of market equilibrium with rational expectations. *Journal of Political Economy*, **90**(2), 223–46.

Newbery, D. and Stiglitz, J. E. (1984) Pareto inferior trade. *Review of Economic Studies*, **51**(164), 1–13.

Radner, R. (1974) A note on unanimity of stockholders' preferences among alternative production plans: a reformulation of the Ekein-Wilson model. *Bell Journal of Economic Management Science*, **5**(1), 181–4.

Rodriguez, A. and Stiglitz, J. E. (forthcoming). Unemployment and efficiency wages: the adverse selection model. Paper presented at NBER/CEPR Conference on Unemployment and Wage Determination, Boston, October 1991. Forthcoming in the Proceedings.

Ross, S. (1973) The economic theory of agency: the principal's problem. *American Economic Review*, **63**(2), 134–9.

Rothschild, M. and Stiglitz, J. E. (1970) Increasing risk: I. a definition. *Journal of Economic Theory*, **2**(3), 225–43.

Rothschild, M. and Stiglitz, J. E. (1971) Increasing risk: II. its economic consequences. *Journal of Economic Theory*, **5**(1), 66–84.

Rothschild, M. and Stiglitz, J. E. (1972) Addendum to increasing risk: I. a definition. *Journal of Economic Theory*, **5**(2), 306.

Rothschild, M. and Stiglitz, J. E. (1973) Some further results on the measurement of inequality. *Journal of Economic Theory*, **6**(2), 188–204.

Rothschild, M. and Stiglitz, J. E. (1976) Equilibrium in competitive insurance markets: an essay on the economics of imperfect information. *Quarterly Journal of Economics*, **90**(4), 629–49.

Sah, R. and Stiglitz, J. E. (1985a) Human fallibility and economic organization. *American Economic Review*, **75**(2), 292-7.

Sah, R. and Stiglitz, J. E. (1985b) Perpetuation and self-reproduction of organizations: the selection and performance of managers. Presented at World Congress of Econometric Society, Cambridge, August.

Sah, R. and Stiglitz, J. E. (1986) The architecture of economic systems: hierarchies and polyarchies. *The American Economic Review*, **76**(4), 716–27.

Sah, R. and Stiglitz, J. E. (1988a) Committees, hierarchies and polyarchies. *The Economic Journal*, **98**, 451–70.

Sah, R. and Stiglitz, J. E. (1988b) Qualitative properties of profit-maximizing *K*-out-of-*N* systems subject to two kinds of failure. *IEEE Transactions on Reliability*, **37**(5), 515–20.

Sah, R. and Stiglitz, J. E. (1991) Quality of managers in centralized versus decentralized economic systems. Forthcoming in *Quarterly Journal of Economics*, February 1991.

Sah, R. and Stiglitz, J. E. (1992) *Peasants versus City Dwellers*. Oxford: Clarendon Press.

Salop, J. and Salop, S. (1976) Self-selection and turnover in the labor market. *Quarterly Journal of Economics*, **90**, 619–27.

Salop, S. and Stiglitz, J. E. (1977) Bargains and ripoffs: a model of monopolistically competitive price dispersions. *Review of Economic Studies*, **44**, 493–510.

Salop, S. and Stiglitz, J. E. (1979a) Monopolistic competition with outside goods. *Bell Journal of Economics*, **10**, 141–56.

Salop, S. and Stiglitz, J. E. (1979b) Strategic entry deterrence. *American Economic Review*, **62**(2), 335–8.

Salop, S. and Stiglitz, J. E. (1982) The theory of sales: a simple model of equilibrium price dispersion with identical agents. *American Economic Review*, **72**(5), 1121–30.

Sappington, D. and Stiglitz, J. E. (1987) Privatization, information and incentives. *Journal of Policy Analysis and Management*, **6**(4), 567–82.

Shapiro, C. and Stiglitz, J. E. (1984) Equilibrium unemployment as a worker discipline device. *American Economic Review*, **74**(3), 433–44.

Shell, K. (ed) (1967) *Essays on the Theory of Optimal Economic Growth*. Cambridge, Mass.: MIT Press.

Shell, K., Sidrauski, M. and Stiglitz, J. E. (1969) Capital gains, income and savings. *Review of Economic Studies*, **36**, 15–26.

Shell, K. and Stiglitz, J. E. (1967) Allocation of investment in a dynamic economy. *Quarterly Journal of Economics*, **81**, 592–609.

Solow, R. (1956) A contribution to the theory of economic growth. *Quarterly Journal of Economics*, pp. 65–94.

Solow, R. and Stiglitz, J. E. (1968) Output, employment and wages in the short run. *Quarterly Journal of Economics*, **82**, 537–60.

Stiglitz, J. E. (1967) A two-sector, two class model of economic growth. *Review of Economic Studies*, **34**, 227–38.

Stiglitz, J. E. (1969a) Allocation of heterogeneous capital goods in a two-sector economy. *International Economic Review*, **10**, 373–90. (Presented at the 1965 Chicago Symposium on the Theory of Economic Growth.)

Stiglitz, J. E. (1969b) Distribution of income and wealth among individuals. *Econometrica*, **37**(3), 382–97. (Presented at the December 1966 meetings of the Econometric Society, San Francisco.)

Stiglitz, J. E. (1969c) A re-examination of the Modigliani–Miller theorem. *American Economic Review*, **59**(5), 784–93. (Presented at the 1976 meetings of the Econometric Society, Washington, DC.)

Stiglitz, J. E. (1970) Factor price equalization in a dynamic economy. *Journal of Political Economy*, **78**, 456–89.

Stiglitz, J. E. (1971) Perfect and imperfect capital markets. Paper presented to the Econometric Society Meetings, New Orleans.

Stiglitz, J. E. (1972a) On the optimality of the stock market allocation of investment. *Quarterly Journal of Economics*, **86**(1), 25–60.

Stiglitz, J. E. (1972b) Some aspects of the pure theory of corporate finance: bankruptcies and take-overs. *Bell Journal of Economics*, **3**(2), 458–82.

Stiglitz, J. E. (1973a) Taxation, corporate financial policy and the cost of capital. *Journal of Public Economics*, **2**, 1–34.

Stiglitz, J. E. (1973b) The badly behaved economy with the well behaved production function. In J. Mirrlees (ed), *Models of Economic Growth*. London: Macmillan, 118–37. (Presented at the International Economic Association Conference on Growth Theory, Jerusalem, 1970.)

Stiglitz, J. E. (1973c) Recurrence of techniques in a dynamic economy. In J. Mirrlees (ed), *Models of Economic Growth*. London: Macmillan, 138–61.

Stiglitz, J. E. (1974a) Growth with exhaustible resources: the competitive economy. *Review of Economic Studies*, March, 139–52.

Stiglitz, J. E. (1974b) The Cambridge–Cambridge controversy in the theory of capital: a view from New Haven. *Journal of Political Economy*, **82**, 893–904.

Stiglitz, J. E. (1974c) On the irrelevance of corporate financial policy. *American Economic Review*, **64**(6), 851–66. (Presented at a conference in Hakone, Japan, 1970.)

Stiglitz, J. E. (1974d) Alternative theories of wage determination and unemployment in LDCs: the labor turnover model. *Quarterly Journal of Economics*, **87**, 194–227.

Stiglitz, J. E. (1974e) Incentives and risk sharing in sharecropping. *Review of Economic Studies*, **41**, 219–55.

Stiglitz, J. E. (1974f) Growth with exhaustible natural resources: efficient and optimal growth paths. *Review of Economic Studies*, March, 132–7.

Stiglitz, J. E. (1975a) The efficiency of market prices in long run allocations in the oil industry. In G. Brannon (ed), *Studies in Energy Tax Policy*. Cambridge, Mass.: Ballinger, 55–99. (Report written for the Ford Foundation Energy Policy Project, August 1973.)

Stiglitz, J. E. (1975b) The theory of screening, education and the distribution of income. *American Economic Review*, **65**(3), 283–300.

Stiglitz, J. E. (1975c) Incentives, risk and information: notes towards a theory of hierarchy. *Bell Journal of Economics*, **6**(2), 552 79. (Presented at Berlin Symposium on Planning, August 1973.)

Stiglitz, J. E. (1975d) Information and economic analysis. In Parkin and Nobay (eds), *Current Economic Problems*. Cambridge: Cambridge University Press, 27–52. (Proceedings of the Association of University Teachers of Economics, Manchester, UK, April 1974.)

Stiglitz, J. E. (1976a) The efficiency wage hypothesis, surplus labor and the distribution of income in LDCs. *Oxford Economic Papers*, **28**(2), 185–207.

Stiglitz, J. E. (1976b) Estate taxes, growth and redistribution. In R. Grieson (ed), *Essays in Honor of W. Vickrey*. Lexington: Lexington Publishing.

Stiglitz, J. E. (1976c) Notes on estate taxes, redistribution and the concept of balanced growth path incidence. *Journal of Political Economy*, **86**(2), part 2, 137–50.

Stiglitz, J. E. (1977) Monopoly, non-linear pricing and imperfect information: the insurance market. *Review of Economic Studies*, **44**, 407–30.

Stiglitz, J. E. (1978) Equality, taxation and inheritance. In W. Krelle and A. F. Shorrocks (eds), *Personal Income Distribution*. Amsterdam: North-Holland, 271–303. (Proceedings of IEA Conference, Noordwijk aan Zee, Netherlands, April 1977.)

Stiglitz, J. E. (1982a) Information and capital markets. In F. Sharpe and C. Cootner (eds), *Financial Economics: Essays in Honor of Paul Cootner*. Englewood Cliffs, NJ: Prentice Hall, 118-58.

Stiglitz, J. E. (1982b) The inefficiency of the stock market equilibrium. *Review of Economic Studies*, **49**, 241 61. (Paper presented at a Conference on Uncertainty and Insurance in Economic Theory in Honor of Karl Borch, Bergen, April 1979.)

Stiglitz, J. E. (1985a) Economics of information and the theory of economic development. *Revista De Econometria*, 5–32.

Stiglitz, J. E. (1985b) Credit markets and the control of capital. *Journal of Money, Banking and Credit*, **17**(1), 133-52.

Stiglitz, J. E. (1985c) Equilibrium wage distribution. *Economic Journal*, **95**, 595 618.

Stiglitz, J. E. (1987a) The causes and consequences of the dependence of quality on prices. *Journal of Economic Literature*, **XXV**, 1–48.

Stiglitz, J. E. (1987b) Human nature and economic organization. Jacob Marashak Lecture, presented at Far Eastern Meetings of the Econometric Society, October.

Stiglitz, J. E. (1987c) Sharecropping. In *The New Palgrave: A Dictionary of Economics*. London: Macmillan. Subsequently reprinted in J. Eatwell, M. Milgate and P. Newman (eds), *The New Palgrave: Economic Development*. London: Macmillan, 308-15.

Stiglitz, J. E. (1988) Economic organization, information, and development. In H. Chenery and T. N. Srinivasan (eds), *Handbook of Development Economics*. Amsterdam: Elsevier Science, 94–160.

Stiglitz, J. E. (1989a) Using tax policy to curb speculative short-term trading. *Journal of Financial Services Research*, **3**(2/3), 101–15.

Stiglitz, J. E. (1989b) Mutual funds, capital structure, and economic efficiency. In S. Bhattacharya and G. Constantinides (eds), *Theory of Valuation - Frontiers of Modern Financial Theory, Vol. 1*. Totowa, NJ: Rowman and Littlefield, 342–56.

Stiglitz, J. E. (1989c) Imperfect information in the product market. In *Handbook of Industrial Organization*, Vol. 1. Amsterdam: Elsevier Science, 769–847.

Stiglitz, J. E. (1989d) Principal and agent. In J. Eatwell *et al.* (eds), *The New Palgrave: Allocation, Information and Markets*. London: Macmillan, 241–53.

Stiglitz, J. E. (1989) Some aspects of a general theory of economic organization. Lecture presented at the Ninth Latin American Meeting of the Econometric Society, Santiago, Chile, August.

Stiglitz, J. E. (1990) On the economic role of the state. In A. Heertje (ed), *The Economic Role of the State*, (Bank Insinger de Beaufort NV). Oxford: Basil Blackwell.

Stiglitz, J. E. (1992a) Prices and queues as screening devices in competitive markets. In D. Gale and O. Hart (eds), *Economic Analysis of Markets and Games: Essays in Honor of Frank Hahn*. Cambridge, Mass.: MIT Press, 128 66. (IMSSS Technical Report No. 212, Stanford University, August 1976.)

Stiglitz, J. E. (1992b) Alternative approaches to macroeconomics: methodological issues and the new Keynesian economics. Forthcoming in A. Vercelli and N. Dimitri (eds), *Alternative Approaches to Macroeconomics*. Oxford: Oxford University Press.

Stiglitz, J. E. (1992c) Contract theory and macroeconomic fluctuations. In L. Werin and H. Wijkander (eds), *Nobel Symposium (No 77) on Contract Economics*. London: Basil Blackwell.

Stiglitz, J. E. (1992d) Capital markets and economic fluctuations in capitalist economies. *European Economic Review*, Vol. 36. Amsterdam: North-Holland, 269-306. (Marshall Lecture, the European Economic Association Annual Meeting, Cambridge, UK, August 1991.)

Stiglitz, J. E. (1992e) W[h]ither socialism?: perspectives from the economics of information. Wicksell Lectures presented at Stockholm, May 1990. Forthcoming, Cambridge, Mass.: MIT Press.

Stiglitz, J. E. and Weiss, A. (1981) Credit rationing in markets with imperfect information. *American Economic Review*, **71**(3), 393 410. (Presented at a meeting of the Western Economic Association, June 1978.)

Stiglitz, J. E. and Weiss, A. (1992) Asymmetric information in credit markets and its implications for macroeconomics. Forthcoming in *Oxford Economic Papers* (Special Issue), **44**.

Index

Abelson, R. P., 119
Aihara, S., 47
Akerlof, G., 156, 175
Albers, W., 130
Allen, F., 169
alternative conceptual frameworks in economic theory, 106
Andreatta, B., 102
anticipatory learning, 133
Aoki, M., 172
Arnott, R., 163, 164, 166, 170–1
Arrow, K. J., 7, 143, 156, 166, 172
  impossibility theorem, 70–1
  Limits of Organization, influence on P. Dasgupta, 28–9
  research, with P. Dasgupta on Rawlsian savings and population ethics, 26–7
Arrow, K. J. and Hahn, F. H., *General Competitive Analysis*, 49
Atkinson, A. B., 6, 148, 156
Aumann, R. J., 67–8, 124–5
Axelrod, R., 119
Azariadis's theory, 55

Babbage, C., 87
Bacharach, M., 103, 104
Baldone, S., 105
Baranzini, M., 105, 107–8
  Foundations of Economics, 106, 108
bargaining under incomplete information, 123–4
Barro, R. J., 151
Basevi, G., 102
Bevan, D., 154, 176
Bevan, P., 154
Bhaduri, A., 2, 4, 5
bilateral markets, 121
Binmore, K., 75
Blackburn, S., 27
Bohm-Bawerk theory, reconstruction by T. Negishi, 58–9
border-pricing rule for tradable goods, 14, 16
Bose, S., 6
bounded rationality, 118–19, 132–5
Brainard, W., 152
Braverman, A., 166, 171
Bray, M., 169
Brown, J. A. C., 103, 104
Bruno, M., 6

capabilities,
  capability network, 95–6
  fund-capability network of pure labour economy, 88–90
  research of R. Scazzieri, 88–91
Cass, D., 152, 153
causal diagrams, 119
chain store game, 132, 133
Chakravarty, S., 4–6, 107
Champernowne, D., 5–6, 10
characteristic function bargaining, 121
Cippa, R., 105
civil liberties, views of P. Dasgupta, 37–8
classical economics, study by T. Negishi, 47–8
cognitive maps, 119

Collery, A., 174
commodity flows, vertical integration, 95
commodity price stabilization schemes, 170
Cournot-Nash equilibrium in multiproduct oligopoly theory, 123
Cripps, F., 2
customs union, 52–3
Cyert, R., 12

D'Adda, C., 102
Dasgupta, P., 1–46, 156
  K. J. Arrow's *Limits of Organization*, influence of, 28–9
  A. Bhaduri, friendship with, 2
  Cambridge University, 14
    see also education
  Carnegie-Mellon University, Faculty visitor, 12
  childhood in Washington, 24
  F. Cripps, friendship with, 2
  P. David,
    friendship with, 19–20
    research with, 22–3
  Delhi School of Economics, 16
  doubts of Classical Utilitarianism as basis for population policies, 27
  economic instruction by M. Kamien, T. McGuire and R. Lucas, 12–13
  education,
    graduate at Cambridge, 1, 2–11, (official supervisor, choice of), 2–3, (– J. Mirrlees), 2–3, 6–7, 8–9, (– H. Uzawa), 3–4, (Research Fellowship at Trinity Hall), 9, (Stevenson Price), 9, (thesis), 3, 7–9, (thesis examiners), 10–11
    schooling, 24–5
    undergraduate at Cambridge, 1–2
    undergraduate at Delhi, 1
  environmental research for WIDER, 32–3
  environmental resources, 30–3
  exhaustible resources, 16–20
  family history, 24
  family relationship, 25
  father,
    academic life, 24
    interest in economics and politics, 1, 25
    work for IMF, 24
  G. Heal, research with, 16–20
  institution design, 28–30
  lectures,
    Frank Paish Memorial Lecture to Royal Economic Society 38,
    Inaugural Lecture at LSE, 30
    Walras–Bowley Lecture to Econometric Society 1989, 35
    Woodrow Wilson School at Princeton, 36
  LSE, teaching post, 16
  malnutrition and unemployment, 33–4
  marriage, 11
  E. Masin, research with, 22
  J. Meade, friendship with, 7
  optimal development with non-transferable capital, 4–7
  patent races, 22
  political philosophy, 23–4
  public investment, 11–14
  Rawlsian savings and population ethics, 26–8
    discussion and research, with K. J. Arrow, 26–7
  D. Ray, research with, 33–4
  science and technology, economics of, 22–3
  Stanford University, 19, 22, 33, 36–9
  J. E. Stiglitz, research with, 14–16, 21, 22, 168
  technological change and market structure, 20–2
  trade, taxation and public production, 14–16
  tutoring by E. Maskin, 29–30
    joint paper with P. Hammond, 30
  UNCTAD/UNEP research programme on environment and development, 30–2
  undernourishment, economics of, 33–4
  UNIDO lectures in Tehran, 12
  US involvement in Vietnam, opposition to, 4
  views,
    K. G. Maler, 32–3
    political and civil liberties, 37–8
    J. Rawls, 26–8
    B. Russell, 25–6
    F. M. Scherer, *Industrial Market Structure and Economic Performance*, 20–1
    J. Schumpeter, *The Theory of Economic Development*, 20–1
    state of poverty studies, 34–6
  well-being and destitution, 38–9

Datta-Choudhuri, M., 12, 175
David, P., 19–20, 22–3, 168
decision theory, application to theory of firm, 117–18
Diamond, P. A., 154, 161
  'Optimal taxation and public production', 15
disequilibrium macro-economics and dynamics, 150–1
Dixit, A., 158
dynamics, economics of, 149–50, 153

economic actions, objective and subjective representations, 98, 99, 100
economic analysis, moral philosophy and political arithmetic approaches, 85–6
economic theory goals, 81–2
economies with imperfect information and incomplete markets, 166
education, higher, relationship to productivity of economy, 154
Enriques, F., 101
environmental resources, 30–3, 167
equal share analysis and coalition formation, 120
equilibrium selection, 128–9
equilibrium wage distributions, 155–6
ESS (evolutionarily stable strategy), 131
exchange, relationship with production, 87, 96–7
exhaustible resources,
  P. Dasgupta, 16–20, 30–3
  J. E. Stiglitz, 167
experimental economics, 117–18

Farrell, J., 169
Farrell, M., 151
Fields, G., 154
flexible manufacturing, 92
folk theorems, 778
Forrester, J., *World Dynamics*, 16
framing, 121
Friedman, M., 143–4
functioning of economy, need for imperfections, 149
fund-capability network of pure labour economy, 88–90
fund-material relationships, 91–2
future development of economics, view of T. Negishi, 62–3

game theory, 21
  chain store paradox, 132, 133
  evolutionary, 130–2
    population genetics, 131–2
  intepretation of equilibrium strategies in sequential games, 69, 77–81
  models of internal relations, 124–5
  multistage models of imperfect competition, 126–7
  non-co-operative, 135
  perfectness, 127–8
  use of word *strategy*, 69, 76–7
  value theory for n-person games, 116–17
  see also Selten, R.
Gaskins, D., 167
general equilibrium theory, 48–55, 174
  contribution by T. Negishi, 49–50
    analysis of stability of equilibrium, 51
    comparison of Walrasian theory and Keynesian economics, 53–5
    introduction of imperfect competition, 50
Gilbert, R., 166
Gioja, M., 87
Goodwin, R., 107
Gordon, H. S., 19
Greenwald, B., 166, 168-9
Grossman, H. I., 151
Grossman, S., 157, 162, 166
growth model, Passinetti, 148
growth theory, 143
Guth, W., 129

Hahn, F. H., 10–11, 19, 51, 143, 147, 149
Hall, R., 172
Hammerstein, P., 130–1
Hammond, P., 30
Harris, J. R., 154
Harsanyi, J. C., 116, 123, 124, 125, 128–9
Hart, O., 162
Hayek, F. A., 172–3
Heal, G., 6, 16–20
Heertje, A., 173
Hendrichs, H., 130
Hicks, Sir John, 96, 103–4
  Causality in Economics, 104
  Methods of Dynamic Economics, 107
  Value and Capital, 48
history of economic theory,
  comments by T. Negishi,
    Adam Smith's value theory, 59–60
    J. S. Mill's wages fund doctrine, 57–8
    Karl Marx and international trade and exploitation, 60–1

history of economic theory,
  comments by T. Negishi (*continued*)
    Ricardo's theory of international trade, 56–7
    von Thunen's natural wage theory, 61
  reconstruction by T. Negishi, Bohm-Bawerk theory, 58–9
  research by T. Negishi, 55–61
Hoff, K., 171
Hoggatt, A., 123–4, 125
Hopcraft, P., 154
horizontal and vertical representations of economic structure, 107–8
Hotelling rule, 17

incentives,
  piece-rates, 163
  theory of, 162–4
income and wealth distribution, 150
infant industry protection, 51–2
information economics,
  critique of perfect competition, perfect information paradigm by J. E. Stiglitz, 159
  information-theoretic approach to market analysis, 159–68
  research, of J. E. Stiglitz, 158–72
  results dependence on assumptions made, 165
  returns of expenditures on information, 161–2
institution design, views of P. Dasgupta, 28–30
insurance market, Rothschild and Stiglitz's model, 29
international trade,
  challenges by T. Negishi to Ricardo's theory, 56–7
  role of exporters and importers, 62–3

Johansson, L., 6
'just in time' production, 92

Kaldor, N., 147–8
Kalish, G., 117, 120
Kalkofen, B., 129
Kamien, M., 12
Kanbur, R., 169
Kemp, M. C., 52
Keser, C., 122
Keynes, J. M.,
  comparison with Walrasian general equilibrium theory, 53–5
  General Theory, 48, 53
Koopmans, T., 3, 152
Kreps, D., 166
Kurz, M., 7

labour,
  division of, 87, 89
    and productivity, 56
labour market,
  screening and adverse selection, 154–5
  wages, 154
Lakatos, theory of scientific research programmes, 55
Lamalfa, G., 175
Landesmann, M., 105, 106–7
Lange, 172–3
language,
  bargaining theory 68, 72–5
  decision theory, 68, 69–71
  theorems about theorems, 68, 71–2
Lavhari, D., 148
Lazear, E. P., 165
Lefebre, L., 144
Leitzinger, J., 171
Leon, P., 102
Levy, F., 175
Liebenstein, 33
Little, I., 12, 13, 14
Loury, G. C., 176
Lovell, M, 12
Lucas, R., 13

McGuire, T., 12
macro-economic equilibrium,
  development of model by J. E. Stiglitz, 168–9
macro-economic theories,
  reconciliation with micro-economic theories, 168–9
Mahalanobis, P., 5
Maler, K. G., 32
Manara, C. F., 101
Marglin, S., 12, 144, 175
mark-up pricing, 96
market behaviour, 157–8
market failure and moral hazard, 166
market socialism, debate between Hayek and Lange, 172–3
Marschak, T., 125–6
Marx, K.,
  comments on international trade and exploitation by T. Negishi, 60–1

  Das Kapital, 60
Maskin, E., 22, 29–30
materials in process, 99–100
Maynard Smith, J., 130, 131
Meacci, F., 109
Meade, J., 7
mean preserving spreads, 153
methodology and research strategies 173–4
Mieskowski, P., 152
Milgrom, P., 172
Mill, J. S.,
  infant industry protection, 52
  wages fund doctrine, 57–8
Mirrlees, J. A., 33, 103, 156, 169
  evaluation of public-sector investment projects in poor countries, 12, 13, 14
  official supervisor of P. Dasgupta, 2–3, 6–7, 8–9
  one-commodity model, 3
  'Optimal taxation and public production', 15
Mitzkewitz, M., 134
model, choice of language, 82
Modigliani, F., 143
Modigliani-Miller theorem,
  reexamination by J. E. Stiglitz, 153
moral hazard theory, 170–1
moral philosophy approach to economic analysis, 85
Morishima, M., 102
multi-profile theorem, definition, 71
Musgrave, R., 152

Nalebuff, B., 163, 164, 165
Nash bargaining theory, 72–5
Negishi, T., 47–66
  Economic Theories in a Non-Walrasian Tradition, 48
  election as member of Science Council of Japan, 48
  general equilibrium theory, 48–55
    alternative proof of existence, 49–50
    analysis of stability of equilibrium, 51
    customs union, 52–3
    infant industry protection, 51–2
    introduction of imperfect competition, 50
    perceived demand curve, 50
    summary of research, 48
    unchanged wages, 55
  General Equilibrium Theory and International Trade, 48
  History of Economic Theory, 48
  history of economic theory, comments on, 55–61
    Karl Marx and international trade and exploitation, 60–1
    J. S. Mill's wages fund doctrine, 57–8
    Ricardo's theory of international trade, 56–7
    Adam Smith, 56
    von Thunen's natural wage theory, 61
    interpretation of Adam Smith's value theory, 59–60
    reconstruction of Bohm-Bawerk theory, 58–9
  Microeconomic Foundations of Keynesian Macroeconomics, 48
  Y. Oishi as undergraduate adviser, 47–8
  presidential address to Japan Association of Economics and Econometrics, 61
  role of exporters and importers in international trade, 62–3
  study of classical economics, 47–8
  teaching by S. Aihara, 47
  views on future development of economics, 62–3
Newbery, D., 156, 170
non-co-operative characteristic function bargaining, 129–30

oil drilling, research of information externalities by J. E. Stiglitz, 171
Oishi, Y., 47–8
oligopolistic general equilibrium, 125–6
oligopoly experiments, 117–23
optimal development with non-transferable capital, 4–7

Pasinetti, L. L., 101, 106
patent races, 22, 167
performance related pay, 165
Perlmutter, A., 125
piece-rate systems as incentive devices, 163
political arithmetic approach to economic analysis, 85–6
political liberties, views of P. Dasgupta, 37–8
population genetics, evolutionary game theory, 131–2
population, optimum and savings, 7–9
population policies, Classical Utilitarianism as basis, 27
poverty studies, views of P. Dasgupta, 34–6

presentation effects, 121
Price, G. R., 130, 131
price rigidities, 169
principal–agent problem, 155
privatization, 172
Prodi, R., 102
production,
  flexible methods, 94–5
  relationship with exchange, 87, 96–7
production process, objective and subjective features, 98–9
productive efficiency, relationship with scale, 98
proportional pay-off bounds, 120
public investment, evaluation, 11–14
pure labour economy, 94–5

Quadrio-Curzio, A., 101, 102, 105–6

Radner, R., 156
Rawls, J., 26–8
Rawlsian savings and population ethics,
  views of K. J. Arrow, 26–7
  views of P. Dasgupta, 26–8
Ray, D., 33–4
Ricardo, D., theory of international trade, 56–7
Ricciardi, F. M., 117
risk, response to, 153
Robinson, J., 2, 3, 103, 146, 147, 148
Rosen, S., 165
Rosenberg, M. J., 119
Rosenberg, N., 168
Rosse, J., 172
Rothschild, M., 29, 153, 155
Rubinstein, A., 67–83
  choice of model's language, 82
  choice of names for concepts, 69, 76–7
  game theory,
    interpretation of equilibrium strategies in sequential games, 69, 77–81
    use of word *strategy*, 69, 76–7
  goal of economic theory, 81–2
  influence of R. Aumann and M. Yaari, 67–8
  language,
    bargaining theory, 68, 72–5
    decision theory, 68, 69–71
    theorems about theorems, 68, 71–2
  social choice theory, 71–2
  undergraduate at Hebrew University of Jerusalem, 67–8

rural organization theory, 171
Ryder, H., 6

Safra, Z., 72
Sah, R., 164, 171
Salop, S., 176
Samuelson, P., 143, 145
Sauermann, H., 117–18
scale, relationship with productive efficiency, 98
Scarf, H., 124, 152, 156
Scazzieri, R., 84–113
  alternative conceptual frameworks in economic theory, 106
  analysis of A. Smith,
    production economy, 93–4,
      (agents–materials network), 93,
      (capability matrix), 93,
      (stock-flow network), 94
    theory of commercial society, 87–8
  analysis of Steuart's description of trade, 86–7
  capabilities, 88–91
  capability network, 95–6
  Catholic University of Milan, 105–6
  childhood in Bologna, 100–1
  economics as path between moral philosopy and political arithmetic, 85–6
  flexible methods of production, 94–5
  Foundations of Economics, 106, 108
  friendship with M. Bacharach, 104
  friendship with R. Cippa, 105
  friendship with Sir John and Lady Ursula Hicks, 103–4
  friendship with F. Meacci, 109
  friendship with L. L. Pasinetti, 106
  fund-material relationships, 91–2
  horizontal and vertical representations of economic structure, 107–8
  materials in process, 99–100
  Oxford University, graduate student, 102–4
  production process, objective and subjective features, 98–9
  production theory, 104
  pure labour economy, 94–5
  relationship between scale and productive efficiency, 98
  relationships between exchange and production, 96–7
  research,
    with M. Baranzini, 105, 106, 107–8

Scazzieri, R. (*continued*)
    with M. Landesmann, 105, 106–7
    with A. Quadrio-Curzio, 105–6
  school education, 100–1
  structural dynamics of economic systems, 107
  structural theory of economic action, 97–100
  time in Cambridge, 106–8
  University of Bologna,
    lectureship, 105–6
    Professor of Economics, 109
    undergraduate, 101–2, (A. Quadrio-Curzio as supervisor), 102
  University of Padua, Professor of Economics, 108–9
  vertical integration, commodity flows, 95
Scherer, F. M., *Industrial Market Structure and Economic Performance*,
  views of P. Dasgupta, 20–1
Schumpeter, J., *The Theory of Economic Development*, views of P. Dasgupta, 20–1
science and technology, economics of, 22–3
Scott, M., 154
Selten, R., 115–39
  anticipatory learning, 133
  application of decision theory to theory of firm, 117–18
  Arms Control and Disarmament Agency, 124–5
  bargaining under incomplete information, 123–4
  bilateral markets, 121
  bounded rationality, 118–19, 132–3
    strategy method, 133–4
  causal diagrams, 119
  characteristic function bargaining, 121
  development of theory of Cournot-Nash equilibrium in multiproduct oligopoly, 123
  early influences, 115–16
  education, University of Frankfurt-on-Main, 115
  equal share analysis and coalition formation, 120–1
  equilibrium selection, 128–9
  ESS (evolutionarily stable strategy), 131
  experimental economics, 117–18
  game theory, 116–17
    evolutionary, 130–2, (population genetics), 131–2
    experiments on five-person apex game, 120
    models of international relations, 124–5
    multistage models of imperfect competition, 126–7
    non-co-operative, 135
    research project *Game Theory in the Behavioural Sciences* at University of Bielefeld, 131
    subgame perfectness, 121–2
    value theory for n-person games, 116–17
  General Equilibrium with Price Making Firms, 126
  habilitation thesis on multiproduct firms, 122–3
  influence of Abelson and Rosenberg, 119
  Institute of Mathematical Economics at University of Bielefeld, 130
  interest in mathematics, 115–16
  non-co-operative characteristic function bargaining, 129–30
  oligopolistic general equilibrium, 125–6
  oligopoly experiments, 117–23
  perfectness, 127–8
  presentation effects, 121
  research,
    with P. Hammerstein, 130–1
    with J. C. Harsanyi, 123, 125, 128–9
    with A. Hoggatt, 123–4, 125
    with T. Marschak, 125–6
    with A. Perlmutter, 125
    under H. Sauermann, 117–18, 122
    with A. Shmida, 131
    with R. Tietz, 124
  support for Esperanto, 125
  visiting Full Professor at University of California, 123
Sen, A., 12, 71, 103
Shapiro, C., 170
sharecropping, 159, 160, 162–3
shareholders, unanimity of preferences, 157
Shelach, S., 68
Shell, K. 149
Shmida, A., 131
Shoven, J., 172
Sidgwick, H., *Methods of Ethics*, 7
Sidrauski, M., 149
Simon, H. A., *Models of Man*, 117
single profile theorem, definition, 71

Smith, A.,
  interpretation of value theory by T. Negishi, 59–60
  theory of commercial society, analysis by R. Scazzieri, 87–8
  Theory of Moral Sentiments, 100
  Wealth of Nations, 56, 87, 92, 100
social choice theory, 71–2
socialism, collapse in Eastern Europe, 172
Solow, R., 18, 143, 145, 151
Spence, M., 166
Sraffa, P., 101, 102
state, economic role, 173
Stern, N., 154
Steuart, J., 86–7
Stiglitz, J. E., 33, 140–83
  analysis of farmer behaviour in Kenya, 155
  Cambridge University,
    graduate student, 146–52
    second visit, 156
  commodity price stabilization schemes, 170
  comparative economic systems, 172–3
  consulting projects,
    Electric Power Research Institute, 167
    Federal Energy Administration, 167
  development of model of macro-economic equilibrium, 168–9
  disequilibrium macro-eonomics and dynamics, 150–1
  distribution of income and wealth, 150
  dynamics of economy, 149–50
  economic role of state, 173
  economies with imperfect information and incomplete markets, 166
  editor of *Journal of Economic Perspectives*, 170
  education,
    Amherst College, 142–3
    graduate student, (Cambridge University), 146–52, (–election to Tapp Junior Research Fellowship), 151, (–supervision by F. Hahn, 147, 148), (–supervision by J. Robinson, 147), (MIT), 143–6
    schooling, 141–2
  equilibrium wage distributions, 155–6
  exhaustible natural resources and energy policy, 167
  father, economic and political views, 141
  growth models, 145
  how markets handle uncertainty, 153
information economics,
  critique of perfect competition, perfect information paradigm, 159
  current work, 158–72
  information-theoretic approach to market analysis, 159–68
  sharecropping, 159, 160, 162–3
  take-over mechanism, 160, 161–2
  theory of incentives, 162–4
  theory of organization of firms, 164
information externalities in oil drilling, 171
Institute for Development Studies in Nairobi, 154–6
intergenerational transfer of abilities, 176
labour market, screening and adverse selection, 154–5
labour market and wages, 154
lectures,
  in Hakone, 157
  in Tokyo on 'Stockmarket allocation of investment', 157
  Wicksell lectures, 173
  world lecture tour, 156–7
litigation consultant, 171
market behaviour, 157–8
market failure and moral hazard, 166
market structure and resource depletion, 21
MIT,
  Assistant Professor, 152
  graduate student, 143–6
monopolistic competition, 158
Oxford University,
  Drummond Professor, 169–71
  sabbatical, 158
Passinetti growth model, 148
price and wage rigidities, 169
privatization, 172
professor at Stanford, 166–8
reexamination of Modigliani-Miller theorem, 153
research,
  with R. Arnott, 163, 166, 170–1
  with A. B. Atkinson (localized learning), 148, (localized technical change), 153
  with D. Bevan, 176
  with D. Cass on economics of dynamics, 153
  with P. Dasgupta, 14–16, 21, 22
  with P. Dasgupta and P. David on

Stiglitz, J. E. (*continued*)
        technical change, 168
    with A. Dixit, 158
    with B. Greenwald, 166, 168–9, 170
    with J. Leitzinger, 171
    with B. Nalebuff, 163, 164, 165
    with M. Rothschild, 153, 155
    with R. Sah, 164, 171
    with C. Shapiro, 170
    with R. Solow, 151
    with A. Weiss, 169, 170
  research assistant at Woods Hole Oceanographic Institute, 143
  Samuelson's editor, 145
  Stanford University, 172
  study of tax policy, 167
  switching controversy, 148
  taxation in developing countries, 171
  technical change, 167–8
    patient race model, 167
  technology flows between North and South, 148
  theory of moral hazard, 170–1
  theory of rural organization, 171
  unanimity of preferences among shareholders, 157
  uncle, political views, 141
  University of Nairobi, 144
  views on methodology and research strategies, 173–4
  views on teaching and collaboration, 174–5
  visits to Bell Labs, 170
  Yale, Assistant Professor, 152–3
stockmarket value, maximizing, 160, 161, 162
structural dynamics of economic systems, 107
subgame perfectness, 121–2
switching controversy, 148

take-overs,
  maximizing stockmarket value, 160, 161, 162
  returns of expenditures on information, 161–2
task network, 99
taxation in developing countries, 171
Taylor, J., 172
teaching and collaboration, views of J. E. Stiglitz, 174–5
technical change, localized, 153
technological change,
  research,
    by P. Dasgupta, 20–2
    by J. E. Stiglitz, 167–8
technology flows between North and South, 148
theory of organization of firms, 164
Thomson, W., 72
Thornton, W., *On Labour*, 57
Tietz, R., 124
Tobin, J., 152, 154
Todaro, M., 154
trade, as organizational structure, 87

Uhlich, G. R., 120, 130, 134
uncertainty, how markets handle, 153
UNCTAD/UNEP research programme on environment and development, 30–2
undernourishment, economics of, 33–4
Uzawa, H., 3–4, 175

van Damme, D., 127
Vanek, 53
von Thunen, comment on natural wage theory by T. Negishi, 61
von Weizacker, C. C., 3

wage rigidities, 169
wages fund doctrine, 57–8
Wakefield, E. G., 109
Walras, L., 48, 49
  general equilibrium theory, comparison with Keynesian economics, 53–5
Weiss, A., 166, 169
Weisskopf, T., 12, 175
Wicksell, 59
Wilson, R., 166
Wolinsky, A., 75, 79–80
worker's capabilities, 87

Yaari, M., 67–8, 152

Zeuthen, F., 74

181 433MD2 BR
10/20/94 40000 4055
ICI
INFORMATION
CONSERVATION INC

The Makers of Modern Economics

Volume I

Arnold Heertje
Professor of Economics, University of Amsterdam

'The book will be claimed as providing a unique insight into the thoughts and lives of prominent economists.... the chapter by Stiglitz is extraordinary. He weaves together the story of how he came to economics, the development of his career, and how he evaluates his own contributions in a masterful manner that one would expect only from a professional novelist, but certainly not an economist.'

Professor David Audretsch, WZB.

In this series Arnold Heertje has invited economists at the peak of their powers to reflect on their influences, education and career as well as the wider concerns of economics as a discipline. What emerges is a remarkable blend of the personal and professional; the ideas and breakthroughs which are shaping the future of the subject are shown to be founded equally on solitary inspiration and the motivation of colleagues and teachers.

The book will be essential reading both for those for whom the experiences might be familiar and for anyone fascinated by the highest level of academic achievement.

Contributors to Volume I:
Partha Dasgupta
Takashi Negishi
Ariel Rubinstein
Roberto Scazzieri
Reinhard Selten
Joseph E. Stiglitz

Forthcoming:
Contributors to Volume II
Masahiko Aoki
Mauro Baranzini
Bruno Frey
Jean-Michel Grandmont
Paul R. Krugman
Edmund S. Phelps
Oliver E. Williamson

Printed in Great Britain

ISBN 0-13-302233-1

9 780133 022339

HARVESTER
WHEATSHEAF

Date Due

MCK DUE APR 08 1997
MCK RTD FEB 25 1997
McK DUE FEB 06 '03
MCK RTD SEP 21 2003
MCK RTD SEP 30 2005

BRODART, INC. Cat. No. 23 233 Printed in U.S.A.

UNIV. OF MD COLLEGE PARK
3 1430 03714459 1

UNIVERSITY OF MARYLAND AT
COLLEGE PARK LIBRARIES